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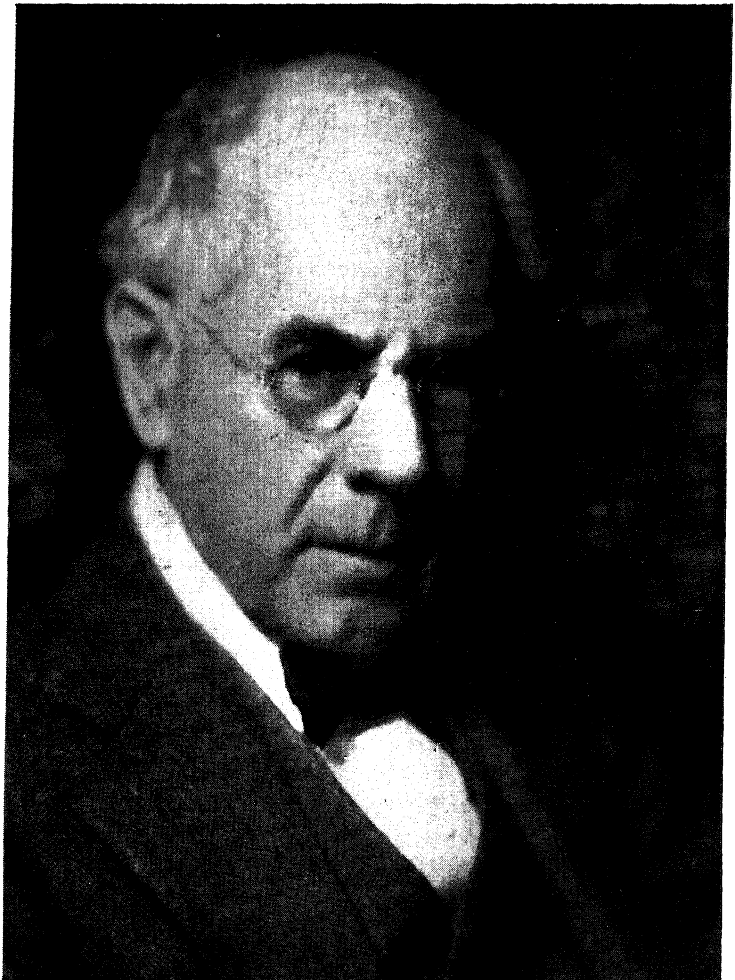

J. McKeen Cattell

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XXV—FIRST MEMOIR

BIOGRAPHICAL MEMOIR

OF

JAMES McKEEN CATTELL

1860–1944

BY

W. B. PILLSBURY

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1947

JAMES McKEEN CATTELL

1860–1944

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Through most of the beginnings and early fruition of scientific psychology, Cattell was a dominant factor in introducing the experimental technique in America. It is true that James and Stanley Hall antedated him; for each the scientific method was grafted upon an older theoretical approach. Neither did much in experiment of the modern type themselves. Hall developed a school of workers, but not quite in the modern method. Cattell on the other hand came in with the experimental method, held the first chair in psychology, and developed one of the first laboratories in America. As editor of journals and head of one of the most productive laboratories in America, he was one of the men who presided over the destinies of psychology for generations of scholars.

James McKeen Cattell was born in Easton, Pa., May 25, 1860. His father was first professor of classics, and from 1863 to 1883 president of Lafayette College. Cattell received his A. B. from Lafayette in 1880, the A. M. in 1883. Following his graduation he studied for two years in Göttingen, Leipzig, Paris, and Geneva, then spent the year 1882-83 as fellow at Johns Hopkins. He then returned to Leipzig, where he studied with Wundt and received the Ph.D. in 1886. Cattell's associations with Wundt were very close during this period. He said once that he told Wundt that he needed an assistant, volunteered for the position and was accepted. He took some credit or blame for Wundt's large literary output, saying that he gave him an American typewriter, without which so much could not have been written.

The period at Leipzig was very fertile in investigation. Six articles were published in the *Philosophische Studien* and several of them were adapted for publication in *Mind* and *Brain* in England. They dealt with various phases of reaction times and association, together with the simpler processes of reading.

All were fundamental to the topics discussed and have been taken as classics in their field.

In 1887 Cattell returned to America as lecturer in psychology at the University of Pennsylvania and Bryn Mawr College. The following year he spent as lecturer at the University of Cambridge. He developed a laboratory there and continued his investigations. This year was especially fruitful for the close contact with Francis Galton, whose methods and ideals Cattell specially admired. They inspired much of his own work in individual differences and in the study of distinguished men that was so important in the middle and later portion of his life. This year he met and married Josephine Owen of London, who was a great help and support all through his career.

The following year Cattell returned to the University of Pennsylvania as chairman of the department of psychology. He established another laboratory and began a vigorous research program. The most important contribution of this period was a study of the perception of small differences in collaboration with Professor G. S. Fullerton of the department of philosophy. The results gave a new turn to the previous interpretations of psychophysics that was influential on the later developments in that field. He showed his usual vigor in putting psychology at once upon a firm footing in the university by the students he attracted and by the respect that he won from colleagues.

Cattell was called to head the department of psychology at Columbia in 1891. He was also acting head of the department of anthropology from 1896 to 1902, and of the department of philosophy from 1902 to 1905. Previous to his appointment the work in psychology had been given in the department of philosophy. Under his direction psychology at Columbia became one of the strongest departments of research and advanced teaching in the country. He gathered about him a group of scholars of the first class. Some remained on the staff, others manned important institutions elsewhere. Forty-six members of the American Psychological Association received Ph.D. degrees during his tenure. He was also in charge of the work

in psychology at Barnard College and for a few years taught in Teachers College. He called strong men to the departments of anthropology and philosophy, so they too prospered through his influence.

As a teacher Cattell left his students much on their own with direction when needed, but with no constant tutelage. They could count on him for admirable counsel when it was needed and he was alert to see that they did not go wrong, but they were encouraged to depend on their own resources and develop their own strength to the limit. The benefit is seen in the later careers of the better men.

From the beginning Cattell took a strong position in the faculty for an independent position of the university professor. He stipulated that he need be at the university for a specific number of days a week and established his residence on top of a hill near Garrison, forty miles from New York. Later he equipped offices and laboratories where he could do his editorial work. This gave him freedom from many of the interruptions of university life. He also contended that many of the administrative problems that dealt with education should be decided by the various faculties and not by deans and the president. This led to many controversies of varied nature. He led a protest against taking a site that had been used as a faculty club for an educational building. A much more elaborate club was secured in consequence, or at least after the protest.

These controversies were probably a factor in Cattell's final separation from the university. The last step came during the war in 1917. Cattell wrote a letter to Members of Congress protesting against sending conscientious objectors to combat duty overseas. The president and trustees interpreted this as an act of treason and dismissed him from the university. In furtherance of his stand for the rights of the professor, Cattell sued Columbia for a large sum for libel. The case was settled by granting a large annuity.

After being freed from university responsibilities Cattell gave much more time to the editorial work that had long taken much of his energies, and to the organization of other scientific

activities. Cattell's first editorship was of the *Psychological Review*, which he founded with J. Mark Baldwin in 1894. He continued as joint editor until 1904, when he sold his share of the enterprise and retired. The *Review* was influential from the first and still continues, having been taken over by the American Psychological Association as its organ. Later he founded the *Archives of Psychology,* which has been edited from the beginning by Woodworth. A companion volume, "The Archives of Philosophy, Psychology, and Scientific Method," lasted but one year but contained much valuable material in the volume published.

Much more demanding in time and administrative ability was the editorship of the series of general scientific and educational journals that Cattell fathered. In 1895 he bought *Science* from Alexander Graham Bell, who had founded it in 1883. Cattell at once made *Science* an important medium for the American scientific world and in 1900 it was made the official organ of the American Association for the Advancement of Science, which relationship still holds as it was given to the Association at his death. In 1900 he acquired from Appletons *Popular Science Monthly,* which he also put upon a successful basis both financially and scientifically. The name was later changed to the *Scientific Monthly.* In 1908 he assumed control of the *American Naturalist.* In 1915 he founded *School and Society* as the weekly medium of communication of American education. It was immediately popular and still continues.

Cattell had a wide range of acquaintances among men of science through his long connection with the American Association for the Advancement of Science and the National Academy of Sciences. His editorial activities with scientific periodicals constantly extended this range. A work, partly a study of the nature and origin of scientific ability and partly a convenient work of reference, resulted in the *Biographical Directory of American Men of Science.* This was unique in that he asked a number of competent men in each field to rate their colleagues in order of merit and then starred a proportion of the total list in each science. The number starred varied

with the number active in each science and was set at a thousand for all the sciences. After the first edition the starred men were asked to select the new men. The volume provided an index for administrative officers of the scientists available. Cattell used the results of estimates for an objective study of the origin and distribution of men of science of different degrees of effectiveness. The directory proved its value on the practical side and the scientific results were highly important. The sixth edition of the work appeared in 1938, and the seventh, under the direction of Jacques Cattell, in 1944. A companion directory, *Leaders in Education,* was published in 1932 and 1940. To print the various journals and books which he had developed into a considerable business, Cattell founded the Science Press in 1923.

In 1921 Cattell took the initiative in founding The Psychological Corporation, a non-profit stock company for the application of psychology to various problems in industry. The stock was held by psychologists and the proceeds were devoted to the advancement of psychological research. Cattell was first president and then chairman of the board. The society has had a long period of usefulness, and tends to increase in the services it performs.

Cattell was very active in many general and special scientific organizations. He watched the course of all that he was connected with very keenly and was always influential in the selection of policies and personnel. He was one of the founders of the American Psychological Association in 1892 and was its president in 1895. He was long a member of the American Association for the Advancement of Science and much of the time chairman of the executive committee; was vice-president of the section in anthropology in 1898 and of the section in education in 1912. He was president of the Association in 1924. He was a member of the American Society of Naturalists (president, 1902), of the New York Academy of Sciences (president, 1902), of the Eugenics Research Association (president, 1914), of the Washington Academy of Sciences (vice-president, 1921), and of Sigma Xi (president, 1913-1915).

He was elected president of the International Congress of Psychology at New Haven in 1929, which by tradition marked him as the outstanding American psychologist of the period. He was a member from 1901 of the National Academy of Sciences, of the American Philosophical Society, and of the American Physiological Society. These honors indicate his eminence in different fields of science.

Cattell's scholarly work shows distinct phases in different periods of his life. His early years, as a student and in the first years of his professorships, were devoted to definitely experimental research, what James called "brass instrument psychology." Then there was a period in which individual differences and the ecology of the man of science dominated his interest. This was in line with the work of Galton, and the Cambridge men, although it was begun some years after the English period of his life. Subsequently, after his separation from Columbia, he continued his studies with tests and in the conditioning of the scientific man and added various other practical applications. Much of this later period was devoted to establishing favorable conditions for the teacher in universities and in facilitating work in research and the publication of the results of research.

Cattell's first study was of the time of exposure necessary to read letters, words, and colors. This divided into two parts in later investigations. One was a measure of the number of simple objects that could be seen with a short exposure, the other the time required to name the object after it was presented. The first series that began the experiments on the span of consciousness were fundamental to later studies of reading. The second problem was a continuation and amplification of the work in reaction times that began with astronomers in their elimination of the personal equation in transit observation and then was taken over by the physiologists and by Wundt and his students.

Cattell's studies of reaction times improved upon his predecessors' in his greater recognition of the importance of deviations from the mean, which had frequently been considered as

less important by the earlier men. He also was not content with the relatively small number of different measurements of the older men. A considerable part of the advance that Cattell made was due to the decided improvements he made in the instruments that were used for presenting the stimuli and in recording the response, especially in speech. The new activities he measured were mainly the times of perception under different conditions and the so-called will time. Possibly most important for later work was the study of the difference in time between free and controlled association. The time required for association had been measured only for free association, and that somewhat roughly. Cattell found that response with abstract words required less time than with the concrete words that were more usual in free association. This was stressed more than a decade later by the pupils of Külpe and Ach.

The second of the classical fields that Cattell worked in was psychophysics. The most important of his studies in the topic was an investigation with G. S. Fullerton, professor of philosophy at Pennsylvania, of "The Perception of Small Differences," published in 1892. This differed from earlier work in the field of Weber's Law both in the methods used and in the formulation of the results, and applied a modification of the method of average error. The main advantage was that each response gave a usable value. When the time permitted for the response was controlled the measurements were superior to those of other methods. The most important result of the investigation was to indicate that accuracy of perceiving differences varied not directly with the absolute intensity, as Fechner had found, but more nearly as the square root of the intensity. Woodworth, after a survey of a large number of investigations by different authors, asserts that the results indicate a range between Weber's Law and the Cattell-Fullerton Law of the square root. Results for some men in certain senses are nearer the Weber formula, some nearer the Weber-Fechner formula. The study showed Cattell's wide interest and originality in a standard field of investigation.

The most immediate influence of the period at Cambridge with Galton was an interest in individual differences and means of measuring human capacities. Most directly an outcome of this interest was the study with Farrand: "Physical and Mental Measurements of Columbia University Students" in 1896. The work applied many known tests to 100 students. It was avowedly a preliminary exploratory investigation. It argued for the accumulation of more data and the desirability and feasibility of measuring individuals in useful ways. It did not receive the general application of Binet's work done ten years later because it did not hit upon a simple way of establishing a scale as Binet did in his age scale or as Cattell did later in his establishment of relative position. This and other more general articles by Cattell in the *Educational Review* and in *Mind,* however, did lead to a series of tests made at the St. Louis Exposition and to many researches in different laboratories in this country and abroad. They initiated an interest in tests.

Cattell's discussions of the problems of university administration were always acute, not to say provocative. His volume "University Control, 1913," and his many articles in *Science* kept the problem in the limelight for university men. He was very active in the formation of the Association of University Professors, which has been a great force in improving the status and tenure of the professor. The thesis that he advanced early and firmly maintained, that the professor should have an effective voice in all university administration, has been widely accepted in the university world since he began his campaign. A considerable part of the change must certainly be ascribed to his efforts.

For more than a half century Cattell held a unique position in psychology and in the editorial work of science in general. For a score of years at the turn of the century he was a force in the organization of laboratory and teaching work in his own science. More students were trained to the doctorate at Columbia than at any other institution during that time. Through his publications and their relation to scientific and educational societies he did much in the same period to improve the morale

of scientists in investigation and in teaching. His personal standing was made evident by his selection as president of the International Congress of Psychology in 1929, more than ten years after he had ceased to be connected with an educational institution.

Cattell was more concerned with method in psychology than with theory. He desired first of all to establish facts and to adopt methods that insured accuracy. He leaned to objective statement before behaviorism came into vogue, but did not specifically accept the latter doctrine when it was proposed, nor ally himself with that or any other school. He always opposed limiting the selection of facts to fit any theoretical presupposition.

Personally he was always classed as a fighter. Wells suggests that it was appropriate that his home was on Mt. Defiance. While it is true that he never hesitated to uphold his views against any opposition, and the more intrenched the opposition, the more delight he took in controverting it, he was essentially a kindly man. He always gave encouragement to younger colleagues and was essentially fair in all his relations. He seldom used the lecture method, preferring the informal discussions of the seminar. He always won the respect and affection of his students and held it throughout their careers. The impress he left upon his students as well as his own contributions gave him a very high standing among psychologists.

Dr. Cattell died on January 20, 1944.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Amer. J. Psychol. = American Journal of Psychology
Ann. Amer. Acad. Pol. and Soc. Sci. = Annals, American Academy of Political and Social Science
Educ. Rev. = Educational Review
J. Cons. Psychol. = Journal of Consulting Psychology
J. Phil. Psychol. = Journal of Philosophy, Psychology and Scientific Methods
Mem. Nat. Acad. Sci. = Memoirs, National Academy of Sciences
New Rev. = New Review
Phil. Rev. = Philosophical Review
Phil. Stud. = Philosophische Studien
Pop. Sci. Mo. = Popular Science Monthly
Proc. Amer. Assoc. Adv. Sci. = Proceedings, American Association for the Advancement of Science
Proc. Amer. Psychol. Assoc. = Proceedings, American Psychological Association
Proc. Nat. Cong. Race Betterment = Proceedings, National Congress for Race Betterment
Psychol. Rev. = Psychological Review
Publ. Univ. Pa. = Publications, University of Pennsylvania
Sci. Mo. = Scientific Monthly

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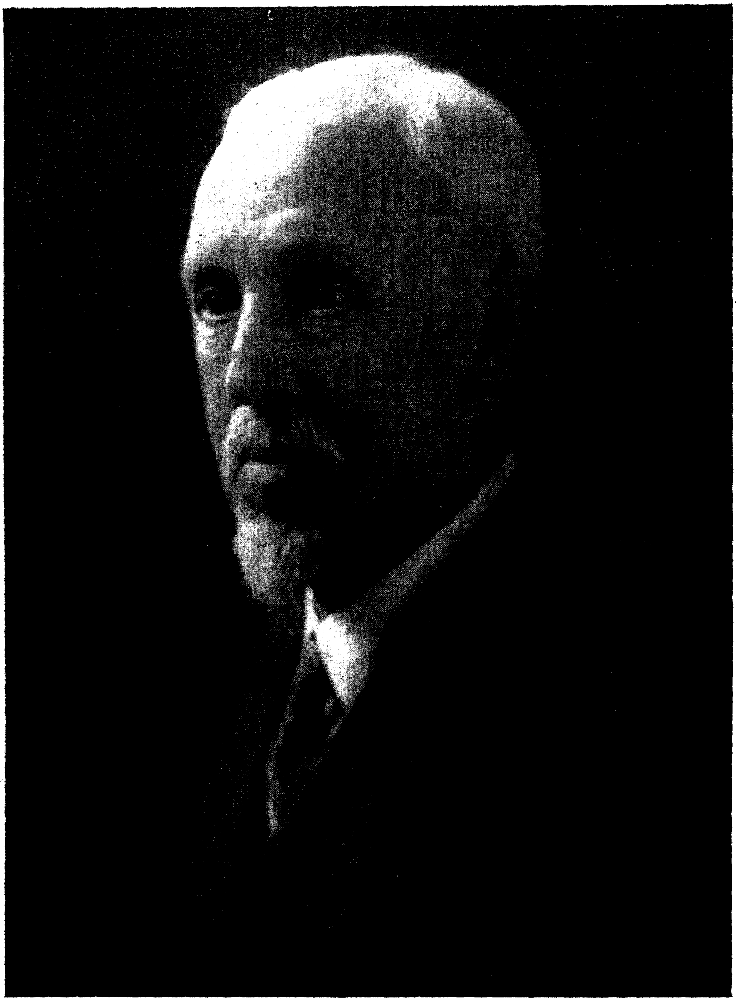

Henry S. White.

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VOLUME XXV—SECOND MEMOIR

AUTOBIOGRAPHICAL MEMOIR

OF

HENRY SEELY WHITE

1861–1943

FOREWORD BY

ARTHUR B. COBLE

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1946

HENRY SEELY WHITE

1861–1943

FOREWORD

Henry Seely White was one of perhaps a dozen men who furnished the inspiration and set the pattern for the development of the present school of American mathematics. His most important personal contribution to this movement was the establishment of the Colloquium Publications of the American Mathematical Society, now in its twenty-eighth volume. A detailed account of his services and of his contributions to mathematics has appeared in Raymond Clare Archibald's Semi-centennial History of the American Mathematical Society in which forty references to Professor White's activities appear.

Professor White was born May 20, 1861, was elected to membership in the National Academy of Sciences in 1915, and died May 20, 1943.

He had deposited in the archives of the National Academy of Sciences a manuscript autobiography. This charmingly modest account of the way of life of a scholar and gentleman of his period is reproduced here with the exception of some references to details of his own work. It is a valuable supplement to more formal histories. Mathematicians particularly will be interested in the picture it presents both of Professor White's personality and of the time in which he lived.

ARTHUR B. COBLE.

A brief account of the life of

HENRY SEELY WHITE

written by himself (1942) for the records of the National Academy of Sciences

My father was Aaron White (1824-97) of Paris and Cazenovia, N. Y., who traced his ancestry to Elder John White and his eldest son Nathaniel, early settlers of Cambridge (Newtown) Mass., later founders of Hartford and Middletown, Conn. Aaron was born at Paris, N. Y., the third of six sons of Roderick White and Lucy Blakeslee. Following the example of his next older brother, Moses Clark White (1819-1900), at the age of 23 he left home and studied at Cazenovia (N. Y.) Seminary and Wesleyan University, Middletown, Conn., graduating with the A. B. degree in 1852. From that date he was a teacher of mathematics, sometimes of physics, mostly at Cazenovia Seminary. There he married in 1859 Isadore Maria Haight (1835-1905), a daughter of William Henry Haight and Cornelia Cushing.

This discloses in my line of descent Enos Cushing, who migrated from Hingham, Mass., and was one of the first settlers in Fenner, Madison Co., N. Y. He lived a long and useful life as land surveyor. Cornelia was one of his four daughters, all of whom lived and died near their birthplace. One of his grandsons, Frank Cushing, attained a reputation as a student of the customs and traditions of the Zuni Indians, and a research member of the Smithsonian Institution.

On my father's side, three brothers went into carpentry, building, and architecture; the other brother died in childhood. Moses Clark, his next older brother, became a physician, missionary to China, and Professor of Pathology and Microscopy at Yale University. Of another brother, Joseph, the eldest son, Andrew Curtis White (1854-1936), graduated at Hamilton College, 1881, and became Instructor in Greek and Assistant Librarian at Cornell University, where he attained the Ph.D. degree in 1888. The five sisters of Aaron White married farmers, some of them teaching in the public schools for a time.

My own two sisters, Cornelia Cushing and Lucy Blakeslee White, were graduated from Cazenovia Seminary; Lucy also from Wellesley College (1891). Cornelia became a librarian, for a time at the Crerar Library, Chicago, and later at Cazenovia Seminary, where, until her death (at age 70), she was also Alumni Secretary. Lucy married Charles Burton Thwing, Ph.D., a physicist, who taught successively at Northwestern University, the University of Wisconsin, Knox College, and Syracuse University, and became finally a manufacturer, chiefly of pyrometers, at Philadelphia, Pa.

Village life was mostly free from excitement, so that school was the chief interest of my early days. Until I entered college, at seventeen, we lived in Cazenovia, Sanquoit, and Canastota, all villages of fewer than 1500 inhabitants. I had begun to read at three, and studied the names on a large Civil War map that hung in the dining room. With improving memory, I learned Bible lessons for Sunday School, and took part in reading aloud a Scripture lesson at daily morning prayers. In my father's private academy at Sanquoit from '65 to '69, and in district school later, arithmetic was my main interest. There also I began algebra under my father's instruction; this was good fortune, for few students have contact with a first class mathematical mind at that early stage. He taught "Natural Philosophy" also, with a fair supply of apparatus, demanding a fair written account of every experiment. At home he let me help a little in preparing specimens for the compound microscope, and introduced me also to rocks and fossils which abounded in those regions. Latin I began at 13, Greek at 14, German at 14. At 17 I had finished preparing for college, having enjoyed the last two years under excellent teachers at Cazenovia Seminary. I doubt whether there was in my secondary school a more efficient teacher of Greek and Latin, for syntax, vocabulary, and memory drill, than Isaac N. Clements, A. M. From college (Wesleyan University) he had gone direct to the army in the Civil War, returned with one wooden leg, and devoted all his active years to teaching. A kindly, sardonic humor and a fine English style made his work doubly

effective. Geometry proved easy and fascinating for me, and in my first year at the Seminary I was successful in the final examination for the Watkins prize in that subject. Learning the use of ordinary drawing tools from my father, I spent my leisure hours in vacation in following out the advanced constructions and problems in the old Young's Geometry, with special interest in regular polygons and polyhedra.

I should have mentioned that my father's good judgment had taken me out of school for half of my fourteenth year, setting me to work at carpentry under the tuition of my uncle Frederick S. White, who contracted for and superintended the construction of a large barn near my home in Canastota. I am confident that this manual training was of value, strengthening my intuition of geometric forms in three-fold space. In other summer vacations, up to the end of my undergraduate course, I worked at all kinds of farm labor on the lakeside farm of my grandfather, Wm. H. Haight, a short distance north of Cazenovia.

At seventeen I entered Wesleyan University, Middletown, Conn., as a freshman, following my father at the interval of thirty years. Greek and Latin were required for two years, also mathematics, which I elected for the last two. Elementary courses were required in physics, chemistry, geology, physiology, and astronomy, to which I added qualitative analysis under Atwater, physics with M. B. Crawford, and practical astronomy with Van Vleck, all teachers of wide learning and experience. I had the good fortune to learn something of Van Vleck's work in preparing the tables of the moon's position for the American Nautical Almanac, thus getting some idea of systematic computation. In the first two years I competed successfully for two mathematics prizes and one in Latin. In Greek I was less fortunate.

Our tutor in psychology and logic was John P. Gordy, later professor in Ohio State University and in New York University. He directed my reading toward Hamilton, Berkeley, Mill, and Spencer, giving me special honors at graduation in philosophy, and a junior year prize in logic, also half of the ethics prize in

senior year. This study of course coordinated well with mathematics, in which Van Vleck gave me a start in quaternions as extra work, and special honors at graduation. My average rank in the whole course was just sufficient for first honor in general scholarship, a rank shared with three others in our class of twenty-seven.

The faculty kindly appointed me Assistant in Astronomy and Physics for my first year of graduate study, with a small stipend. I joined undergraduate groups in conic sections and in Hume's philosophy, assisted in preparations for observing the transit of Venus (1882), operated the 12-inch equatorial for undergraduates, and took series of observations for latitude with the meridian transit instrument. Also I assisted my father in collecting scientific notes for the *Northern Christian Advocate* (Syracuse). For mathematical reading I began the Theory of Equations (Burnside and Panton) and Muir's book on Determinants. Under Van Vleck's supervision, with a circle of seniors, I read part of Gauss's Theoria Motus Corporum Coelestium.

This busy and profitable year was broken up in the last three months by teaching, as substitute for my classmate George Prentice, ill health compelling him to resign the professorship of mathematics at Hamline University, St. Paul, Minn.

During the year 1883-4 I taught mathematics and chemistry, with short courses in physics and geology, at a secondary school, Centenary Collegiate Institute, at Hackettstown, N. J. This gave me valuable pedagogic training under a veteran principal, Rev. Geo. H. Whitney. To his recommendation I owe, in some measure, my appointment for the following three years (1884-7) as Tutor in Mathematics and Registrar at Wesleyan University, my Alma Mater. My duties included class room teaching and field practice in land surveying. In the scant leisure time I continued my latitude observations and some computation work with Van Vleck. During one summer vacation, 1884, I made the decennial "charter survey" and map for the village of Manlius, N. Y., and thought hopefully of the surveyor's profession. But the advice of my close friend Wm. J. James and of Professors Van Vleck and Crawford roused my ambition

for advanced study of elliptic functions, using the book of Durege, and a visiting fraülein gave me conversation lessons in German (1886-7).

My first intention had been to study mathematics in the University of Leipzig, where Sophus Lie was then developing his Gruppentheorie, and E. Study was lecturing on algebra. But after one summer there, devoted principally to language practice and grammar with a law student from Braunschweig, I shifted to Göttingen, enjoying a foot tour through the Harz Mountains on the way. This I did again under the advice of William J. James (later Instructor in Mathematics and for many years Librarian and Assistant Treasurer at Wesleyan University), who had already studied four years under Felix Klein at Leipzig and Kronecker and Fuchs at Berlin. He held up Klein as not only a leading research mathematician but also as a magazine of driving power, whose students received personal attention and stimulus, and in most cases became themselves productive investigators. This was valuable advice. Klein received me kindly and admitted me to his seminar course, then just beginning, in Abelian Functions. Other Americans working with him at the time were Haskell, H. D. Thompson, H. W. Tyler, Osgood of Harvard, and a year later Maxime Bôcher, all friendly and helpful to the less experienced neophyte. Bolza, Maschke, and F. N. Cole, all whose names are now classic in American mathematics, had left the year before. Klein expected hard work, and soon had in succession Haskell, Tyler, Osgood, and myself working up the official Heft or record of his lectures, always kept for reference in the mathematical Lesezimmer. This gave the fortunate student extra tuition, since what Klein gave in one day's lecture (two hours) must be edited and elaborated and submitted for Klein's own correction and revision within 48 hours.

Within six months we were each set to work on special topics and reading related papers, all in close connection with the seminar and lecturing on our findings before the whole seminar group. After my second year, results proving plentiful, Klein set me to formulation for publication and to reviewing for ex-

amination. He kindly published in the *Mathematische Annalen* a condensed account of my problem and chief results (vol. 36), and later procured the insertion of my Thesis *in extenso* in the *Nova Acta* (vol. 57) of the Leopold-Karoline Academy of Sciences at Halle.

Meantime I had followed lectures by H. A. Schwartz, his regular course on function-theory; by Schönflies on projective geometry and curve-theory; by Georg Elias Müller on psychology and *naturphilosophie,* and Baumann on history of philosophy. Müller used me also for a subject in his new psychological laboratory, and helped me with a seminar lecture on Hume's "Untersuchungen über den menschlichen Urstand" (a translation). But equal profit also accrued from my daily study of Clebsch's Vorlesungen über Geometrie (by Lindemann), and Reye's Geometrie der Lage. Those both contributed a secure background to the university lectures. The biweekly sessions of the Mathematische Gesellschaft also were open to us. Kant's *Kritik der Reinen Vernunft* of course was prescribed reading, and in this I had the good fortune to combine with the late John C. Schwab, later Professor and Librarian at Yale, who was preparing for his degree in economics.

My examination came early in March, 1890, with Klein and Müller as chief examiners. Psychology was my second department. The Pedell, conveying keyhole information, calmed my apprehensions by citing the advocacy of those two good friends, and I suppressed as useless the regrets over the more brilliant success that I might have secured by more extended coaching. To the training and encouragement of Felix Klein and G. E. Müller I owe a large part of my good fortune in attaining both desirable teaching positions thereafter, and the "equanimity" (Sir Wm. Osler's name for it) and intellectual satisfactions that accrued in my long experience as teacher and investigator. Many American students can testify similarly of their indebtedness to Klein; not so many to Müller's influence, since psychology had in those years fewer devotees. Yet he was a worthy successor to Lotze and Herbart, and produced work of permanent effect in renovating his chosen science.

Returning home in March, 1890, I took for the spring quarter a temporary position in the preparatory department of Northwestern University at Evanston, Ill., secured for me by my friend and classmate Joseph R. Taylor, who taught Greek in the same school. (He afterwards filled for more than forty years a professorate at Boston University).

Clark University was opening in that year its career, full of high aims and hope, at Worcester, Mass. Its president, Dr. G. Stanley Hall, had spent the previous year visiting universities and scholars in Europe, and looking up available men for his institution. Klein had introduced me and given some flattering testimonials, and Dr. Hall offered me now a place as Assistant in Pure Mathematics under Prof. Wm. E. Story, with Henry Tabor and Oskar Bolza as colleagues. Though the salary was hardly adequate for subsistence, I accepted it eagerly in spite of kind offers from Evanston and Middletown. My teaching was mainly algebraic and projective geometry, and the invariant-theory of linear transformations.

In 1891 I refused an invitation to do similar work at Johns Hopkins University, but in 1892 accepted an associate professorship at Northwestern. The inducements were, first a better salary with assured permanency, and second, proximity to the new University of Chicago and my highly valued friend E. Hastings Moore, its new Head Professor of Mathematics. He indeed tried to bring me into his department but could not secure sufficient appropriation. For my labors at Clark I had secured two short papers that were accepted by the *American Journal of Mathematics,* and a considerable number of new friends among our ambitious younger mathematicians. Some few new problems, too, I had begun to formulate. In particular, in the seminar of Professor Wm. E. Story we had discussed the epoch making paper of David Hilbert of Königsberg (later Göttingen) on Algebraic Forms, embodying the theorem never before enunciated in so general form, that every given sequence, finite or infinite, of algebraic forms in any domain of rationality must have a *finite* number of such forms constituting a "Basis" in terms of which all the rest are linearly expressible with entire

algebraic forms as coefficients. Gordan's proof of the finiteness of the form-system of covariants of any set of binary ground forms was subsumed as a primary corollary. Along with this, Story presented the enumerative work of Sylvester and Franklin on the same (binary) subject. This was fundamental and fruitful.

The new president of Northwestern University, Henry Wade Rogers, had called a number of younger scholars to important chairs, all of about the same age, men with the Ph.D. degree, trained at Johns Hopkins or Harvard or in German universities; and the "atmosphere" was favorable to research and writing. Most of the older professors were of similar fiber, and the college enjoyed an era of marked growth.

My second year in Evanston, 1893-4, was memorable for the Columbia World's Fair, held at Jackson Park on the lake shore at the southern end of Chicago; and for the so called "Congresses" held in connection with it, mostly, at the Fine Arts Building on the lake front at Randolph Street. I served on the committee for the Congress of Mathematics, along with the three professors from the University of Chicago: Moore, Bolza, and Maschke. Professor Felix Klein was deputed by imperial authority to accompany the German exhibit of mathematical books, photographs and apparatus, and he graciously accepted the hospitality of my wife and myself, commuting daily by railroad to the city, 12 miles. A full account of this Congress and the participants was published by the Macmillan Company for the Mathematical Society. Klein gave in the following two weeks twelve lectures, chiefly on the topics covered in his courses at Leipzig and Göttingen. It had been planned that these should be given for one week at the University of Chicago, and one week at Northwestern University. But poor drains and wet weather had flooded the campus at Chicago and all were transferred to Evanston. The University furnished auditorium and reference library and opened a dormitory on the campus for the convenience of listeners. Professor Alexander Ziwet of Michigan kept a full report, which with Klein's emendations was published under the title "The Evanston Colloquium," and

achieved wide circulation. A French translation also had a large sale. The whole series, with the Congress and the exhibits at the Fair, gave a strong stimulus to mathematical research and reading in this country.

In the comparative leisure of the following year, '93-'94, I worked out a theme and a paper for the *American Journal of Mathematics,* for Semi-combinants and Affiliants (this latter term a new one). . . .

In the next ten years I worked out some problems of minor interest, connected with plane cubic curves, elliptic functions, and apolarity. Extending the Euler polyhedron theorem to surfaces of positive deficiency, I tabulated regular systems of divisions upon them, admitting of course curved lines as boundaries. These were based on existence theorems due to Riemann, concerning canonical "Querschnittsystems." The distinction of primitive and derivative systems proved interesting, and I made models, with the help of Professor O. H. Basquin, of all regular sets of divisions upon surfaces of low deficiency. Of my interest in the American Mathematical Society there is a fairly full account in the history, by Professor R. C. Archibald, of the first fifty years of that Society. Chiefly useful was the scheme for holding frequently a colloquium, or week of advanced lectures on special theories, in connection with summer meetings of that Society. The first was held in '96 and I was asked to conduct a course in 1903, this was published as part of "The Boston Colloquium," by the American Mathematical Society, the first of an extensive series. Osgood, Bolza, Bocher, Pierpont, and Webster, by their generous labors, gave this Colloquium series a high standing at the start and made it a valuable feature of American mathematics.

Becoming interested in the work of Picard and Poincare on integrals upon algebraic surfaces, an extension of Riemann's work on Abelian Functions to functions of two independent variables, I secured leave of absence for a half-year's study in Europe, Feb.–Aug. 1901. With this I combined the plan of writing an introductory book on Plane Cubic Curves, a long task which was not completed until 1925. By Klein's advice,

I went to Turin and listened to part of a course by Professor Corrado Segre upon Algebraic Surfaces, treated by synthetic geometric methods. This required a hasty acquisition of some slight speaking knowledge of Italian, a valuable addition to my range. In Turin also I enjoyed a slight acquaintance with D'Ovideo, Peano, and Severi. After the close of the summer semester there, I spent the balance of my free period in Göttingen, busied principally with my manuscript on Cubics. Returning via Liverpool and Boston, I saw for the first time a few points in England, a brief glimpse of London and Cambridge, with a bicycle tour through Oxford, Stratford, Nantwich, and Chester (with Hawarden). This was the third such tour I had enjoyed, bicycles being then at the acme of their popularity before the advent of automobiles. The first two were shared with my friend E. H. Moore, one from Michigan City via Indianapolis, Brookville, Cincinnati to Columbus (1899) and return via Marion, Lima, and South Bend; the other from New York (1900) to Hudson, Great Barrington, Williamstown, Amherst, Worcester, Boston, Providence, and New Haven. Some few other trips I had made alone, far less pleasant and profitable.

During the decade, 1890-1900, occurred my marriage to Mary Willard Gleason of Hartford, whom I had known in college days at Middletown, daughter of the banker Frederic Lathrop Gleason; and the birth of my three daughters. The very efficient domestic and social ability of my wife, a musical and poetic genius, furnished stimulus and sheltered environment for my professional activities. Whatever we accomplished in this ideal partnership was due very largely to her ambition and management.

In 1905, in order to be nearer the home of my mother, who was seriously ill, I accepted the offer of the headship of the mathematics department in Vassar College, under the presidency of Dr. James Monroe Taylor. This availed little, so far as my mother was concerned, for she died during that summer, before we had settled in Poughkeepsie. Besides the usual undergraduate courses we conducted advanced courses and research

with a few graduates who were attracted by endowed fellowships. After a few years, however, the trustees altered the conditions, believing it more profitable for graduate students to pursue such studies in larger groups at some of the larger institutions which were then in process of evolution. Our small departmental staff, however, cooperated effectively in promoting advanced reading and research.

My chief interest, increasing for many years after 1910, was in so-called Triad Systems and their groups. This subject became of importance in the theory of substitution-groups on a finite number of elements, either $6n + 1$ or $6n + 3$, where n is an integer. Up to $n = 2$ there is only one way to arrange the triads (in a set of 3, 7, or 9, elements) ; but for 13 elements it was known that two different, dissimilar arrangements are possible. The definition prescribes namely that every possible pair of the given elements shall occur in one, and in no more than one, triad of the system. Two systems are considered duplicates when each can be derived from the other by simply re-naming the elements. Beyond this, Moore had proved that for all larger values of n there are at least two different triad systems, distinguished by non-conformable groups. . . .

A geometrical application that proved to be important involved duality in three-space. Seven elements of a triad system being represented by points, the triads may naturally be pictured as planes, each determined by 3 points. Now 6 points determine a twisted cubic curve which contains them. If the seventh point of a set lies also on that curve, I found that the seven planes representing any triad system on those points will all osculate a second twisted cubic curve. This gives a theorem for 7 points of a gauche cubic, analogous to the Pascal theorem for 6 points of a plane quadric curve. But further, the two sets, points and planes, have the poristic property. With one degree of freedom, the 7 points may move along their cubic in such a way that the 7 planes continue to osculate their cubic curve. This gives us a link between two gauche cubics, quite similar to Poncelet's polygons connecting two conics in a plane. Coble (A. B.) gave an extensive theoretic treatment of this situation,

calling it the only important generalization of Poncelet's problem. (See citations for both Coble and White in Archibald's History, l.c.).

Other brief geometrical studies have been intended rather to fill gaps or to supplement the work of earlier writers than to open new lines of research. Thus Hurwitz's theorem on two tetrahedrons inscribed to one twisted cubic and circumscribed to another, was extended to two $(n + 1)$-gons inscribed to a norm-curve in n-space. A well known set of points in 3-space are the 8 points in which three independent quadric surfaces intersect; from any seven of them the eighth can have its coordinates calculated rationally. Though this was proven, the explicit formulae had not been given. The result of somewhat tedious labor revealed new relations.

Curiously enough, while algebraic curves and surfaces have been staple objects of study, the degenerate forms, sets of planes or lines, have been neglected, beyond five lines in a plane or six planes in three-space. With Miss Cumming's cooperation I examined the divisions that could result from 6 or 7 real lines, or from 7 real planes. The methods were not less interesting than the results. More recently I have found special interest in skew hexagons and their close connection with the permutations of six marks or symbols. The desideratum is, to find means of skeletonizing the picture, or substituting a framework for the less easily apprehended, fully explicit intuition. For example, in the case of polyhedral division of space by planes, faces of fewer than 5 sides can be ignored in the diagram; i.e., only pentagons and hexagons need be studied with their connectivity.

In 1933, at the age of 72, I was, nominally at least, relieved of part of my teaching obligations, being made Professor Emeritus and Senior Lecturer. Three years later the latter title was withdrawn. Since that time I have been living on a small farm with the family of my youngest daughter, Mary White Perez, devoting some thought to gardening and apple culture, and to admiration of the noble range of the Catskill Mountains.

What further work or experience I may have that will be

worth noting, I cannot foresee. A list of honors of various kinds that have fallen to me is given in Archibald's History, l.c. It omits the compliment of an assignment to write two articles for the Encyclopædia Britannica. (1926).

After my decease, if the custom of printing memorial biographies is continued, I think some competent reporter or friend can extract and condense from this rambling narrative three or four pages which will suffice.

April, 1942.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

AJM = American Journal of Mathematics

AM = Annals of Mathematics

AMM = American Mathematical Monthly

AMS Bull. = American Mathematical Society Bulletin

AMS Colloq. Pub. = American Mathematical Society Colloquium Publications

AMS Trans. = American Mathematical Society Transactions

Astrophys. J. = Astrophysical Journal

Enc. Brit. = Encyclopædia Britannica

MA = Mathematische Annalen

NAS Mem. = National Academy of Sciences Memoirs

NAS Proc. = National Academy of Sciences Proceedings

Sci. Mo. = Scientific Monthly

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W. W. Campbell

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WILLIAM WALLACE CAMPBELL

1862–1938

BY

W. H. WRIGHT

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1947

WILLIAM WALLACE CAMPBELL

1862–1938

BY W. H. WRIGHT

William Wallace Campbell was born on a farm in Hancock County, Ohio, on April 11, 1862. He was the son of Robert Wilson and Harriet Welsh Campbell. The Campbell family, of which Wallace was the youngest except for a son who died in infancy, consisted of three sons and three daughters, none of whom survived him.

Robert Campbell was of Scotch descent. In 1745 or 1746, a time of great unrest in Scotland, some Campbells emigrated from there to County Down, Ireland. In 1785 seven brothers of one Campbell family sailed for America, and settled in Western Pennsylvania, probably Washington County. In 1838 James Campbell and his wife (Jane Wilson) moved from Pennsylvania to Hancock County, Ohio. Their third child, Robert Wilson, married Harriet Welsh, also from Western Pennsylvania. Harriet had come with her family to Ohio in 1837, when she was a child of about eleven, and it is related that she walked most of the way. Robert Wilson Campbell and Harriet Welsh had seven children, of whom, as already has been said, William Wallace, or as he came to be called, Wallace, was the sixth.

Robert Campbell died in 1866, and Wallace had no recollection whatever of his father. The mother had six children to rear, and did it successfully at the cost of very hard work. She appears to have been a person of exceptional intelligence, though she had little opportunity to acquire a formal education. She is reported to have "done arithmetic in her head", that is to say, without recourse to pencil and paper, a circumstance which seems worth noting because her son had the faculty of carrying on calculations with a minimum recording of figures. He not uncommonly looked up two or more logarithms, added them mentally, took out and set down the product—a feat which always excited wonder on the part of one who has no talent whatever for arithmetical computation. Campbell's earlier scientific work was in computational astronomy, and it is hardly to be

doubted that his ease with numbers had much to do with his success in that field.

Robert Campbell was an expert carpenter and cabinet maker, the quality of his workmanship being attested by furniture of his construction still in the possession of his son's family. While in his mental traits Wallace appears, in the opinions of those who knew him as a boy, to have favored his mother, there seems to be little room for doubt that he inherited substantially from both of his pioneer parents.

As a child Wallace had to work very hard. "When he was quite little he had to do chores on the farm, and help hoe the vegetable garden." In later years his wife voiced the suspicion that this may have been the cause of his total lack of interest in gardening: "He loved flowers if someone else would raise them." Be that as it may, one can but assume that Campbell's early experience on the farm had much to do with the formation of that habit of industry which was so conspicuous a trait of his developed character.

Wallace attended the local schools and was much indebted to one of his high-school teachers, a Miss Abbot, for sympathy and encouragement. She recognized his ability and urged him to attend a university of recognized standing, rather than one of the small colleges which are so plentiful in Ohio. After a year or two of school teaching he was able to act on this advice, and in 1882 he applied for admission to the University of Michigan as a student in civil engineering. Here he encountered difficulties deriving from the low standards of the schools he had attended, but the committee of the faculty having to do with such matters decided to give the boy a chance, and the University can hardly have had occasion to regret their action in admitting him.

Campbell was graduated by the University of Michigan in 1886, with the degree of Bachelor of Science, but it is here necessary to step back a little to record an incident of his undergraduate experience which decisively affected the course of his life. One day, during his third college year, while reading in the University library, he happened upon a copy of Simon Newcomb's *Popular Astronomy*. His interest was so captured by the

opening paragraphs of the book that he read it through in two days and two nights. In later years he was accustomed to tell his friends that then and there he "discovered" astronomy and decided to make it his life's study. Under the guidance of Professor J. M. Schaeberle, who was in charge of the University observatory, he became a skillful observer, and after devoting a vacation period to the reading of Watson's *Theoretical Astronomy,* he undertook with success the calculation of comet orbits. During his last year at the University he served as an assistant in the observatory. For two years after graduation, Campbell was Professor of Mathematics in the University of Colorado. While there he met Elizabeth Ballard Thompson, a student at the University, to whom he was afterward married.

In 1888 Professor Schaeberle resigned his position at Ann Arbor in order to join the staff of the Lick Observatory, which opened for operation in the summer of that year. A vacancy was thus created in the department of astronomy at Michigan, and the University invited Campbell to come as instructor. This offer provided an opportunity for him to enter his chosen field, and at a very considerable financial sacrifice he accepted it. During the summer vacation of 1890 Campbell served as voluntary assistant at the Lick Observatory, and helped Astronomer James E. Keeler in his spectroscopic observations. Shortly thereafter Keeler left Mount Hamilton to accept the directorship of the Allegheny Observatory, and two young astronomers, Wallace Campbell and Henry Crew, were invited to come to the Lick Observatory to engage in spectroscopic work. Crew left after a year's service, to become Professor of Physics at Northwestern University, while Campbell remained in charge of the spectroscopic work of the Lick Observatory.

Campbell received his appointment on May 12, 1891. Inasmuch as he was then entering upon the work for which he subsequently became distinguished, it seems appropriate to sketch briefly the state of astronomical spectroscopy at that time. The capital discovery by Kirchoff and Bunsen of the law of spectral absorption that bears the former's name, which law provides the basis of spectroscopic analysis, had been made only 31 years

before. In the interim much valuable work, which, viewed through the frame of present knowledge may be described as largely of a qualitative character, had been accomplished, both in the spectroscopic analysis of the heavenly bodies and in the measurement of their velocities through the employment of the Doppler-Fizeau principle. It so happens, however, that the measurement of a photographic plate for either of the mentioned purposes must be accomplished with extraordinary accuracy, and, with the use of the stellar spectrographs of that day, the error of measurement was in the majority of cases of the order of the quantity sought. These comments are not to be regarded as generally applicable to spectroscopic procedures of the era of which we are speaking, but as relating particularly to astronomical spectroscopy where, because of the faintness of starlight, and of other circumstances of astronomical observation, a less degree of precision had been obtained than in general laboratory practice. Faintness of a stellar spectrum usually necessitates a long photographic exposure, while the spectrograph, being attached to a moving telescope, is subject to disturbance by differential flexure at various stages of the exposure; furthermore, the effects of changing temperature must be guarded against. Generally speaking, the instruments available fifty and more years ago served in measuring relatively large radial velocities, but were useful only in a limited way in the study of the velocities of the great majority of stars.

An exception to the above restrictive comment should perhaps be made in the case of some visual observations made in 1890-91 by Keeler, of the velocities of the more conspicuous nebulae whose spectra consist predominantly of emission lines. Keeler took advantage of the fact that the bright lines are not weakened by the employment of high dispersion, and was thus, in respect to this particular group of objects, able to make observations which conform to present standards of accuracy, but his procedures were not applicable to the study of the vastly more numerous stars. He had in mind the construction of a spectrograph to undertake the larger work, but was unable to realize his desire until after he moved to the Allegheny Observatory.

It was in this investigation that Campbell had assisted Keeler during the period of his voluntary service at the Lick Observatory in the summer of 1890. Campbell's early association with Keeler cannot be regarded as other than fortunate both for himself and for the Lick Observatory.

It is convenient for the purpose of this biographical summary to divide Campbell's active association with the University of California into three periods. These are: first, the initial period of approximately ten years, 1891 to 1901, during which he held the position of astronomer in the Lick Observatory; second, the interval 1901 to 1923, during which he was director of the Observatory; and third, 1923 to 1930, when he filled the offices of president of the University and director of the Observatory. There followed a fourth period, that of his retirement; this included his service as president of the National Academy of Sciences.

During the first of these intervals he was extraordinarily active as an observer, and did the greater part of his observational work. He had hardly settled himself in his new position at Mount Hamilton when a brilliant "new" star flashed in the heavens: Nova Aurigae of 1892. This was by far the brightest nova that had appeared since the employment of the spectroscope in astronomical observation, and little was known of the nature of a nova spectrum. A new field was opened to the young astronomer, and he entered it boldly with everything he had, which, however, was not a great deal, judged by modern fashions in spectroscopic equipment. It is true that he had the use of a powerful telescope, but his spectroscope, the one used by Keeler in observing the nebulae, was not an instrument suited to rapid observation or to faint objects. The spectrum of a nova changes from hour to hour, and sometimes alters its character altogether from night to night, so that speed is essential to successful observation. Moreover, the spectroscope was designed for visual use, while most spectroscopic observations, even at that time, were made photographically. Nevertheless, Campbell made most creditable observations of the star and, by dint of observing it as early as practicable in the morning sky

when it again came into observation, he showed that its spectrum had lost its continuous character, and resembled that of a gaseous nebula. Equally important were the observations made by him at about that time of the spectra of nebulae and of Wolf-Rayet stars. Some of Campbell's findings were so surprising as to excite incredulity, and to bring upon him from certain quarters sarcastic and bitter criticism. To mention an instance: he reported a variation within the Orion nebula of the relative intensity of the F line of hydrogen on the one hand, and the bright nebular lines, then known as N_1 and N_2, on the other. Variations in the relative intensity of various lines within the body of a single nebula are now well known, and provide the basis for determining some of the physical conditions within the nebulae, but Campbell's observation was the first of its kind, and was, for a time, lightly treated by a highly placed critic. (97) (103) (113) (114); *cf Astroph. Jour.*, 10, 164, 167.

In those early days Campbell had a number of controversies with astronomers who were disposed to question his findings, especially when these did not conform to currently accepted views. He was a very careful observer, and when he was ready to describe an observation he was confident of its validity. On the other hand he was young, his name was a new one in astrophysical literature, and this may have served to divest his critics of due and reasonable caution. Eventually they seemed to become more circumspect. Perhaps his most stubbornly defended conclusion was that water-vapor and oxygen are relatively scarce in the atmosphere of the planet Mars. His first observations were made at the favorable opposition of the planet in the summer of 1894. At that time Mars, and the possibility of its being inhabited by intelligent beings, occupied a large part of the public's interest in astronomy, and even conservative astronomers had been won to the view that the planet had an atmosphere, with water vapor and oxygen as constituents, that is to say an atmosphere which inferentially is capable of supporting life. In Young's *General Astronomy* (Edition of 1891), the text book most commonly used in American colleges of the day, occurs the statement, in relation to the atmosphere of Mars:

"Dr. Huggins has found with the spectroscope unequivocal evidence of the presence of aqueous vapor." This may fairly be taken as the accepted view of that period. For reasons that need not be given here, the spectroscopic detection in the atmosphere of a planet of a vapor, such as oxygen or water-vapor, that is also present in the earth's atmosphere, is one of the most difficult tasks in the astronomical spectroscopy. Campbell was alive to the difficulties and exercised the most painstaking care to minimize them. His conclusion, based upon his own observations, was that if water vapor or oxygen occur in the atmosphere of Mars it is in quantities too small to have been detected by spectroscopes then available. This conclusion ran so counter to the view generally accepted at the time that it was sharply criticized by some astronomers, more especially by those who were disposed to interpret the phenomena observed telescopically on the surface of Mars in terms of a rather tight analogy with what happens on the earth at this particular stage of our civilization and economic development. The controversy continued for some years, and even so late as 1909 observations were published which were interpreted by their sponsors as proving the very considerable occurrence both of water-vapor and oxygen in the atmosphere of Mars.[1] Campbell returned to the problem in 1909. In order to minimize the effect of the earth's atmosphere an elevated observing station seemed desirable, and he conducted an expedition to the summit of Mount Whitney, elevation 4420 meters, the highest point in the continental United States, to make his observations. The methods used on that occasion were similar to those that had theretofore generally been employed, but in 1910 he attempted to solve the problem in a different way, namely, by observations of Mars when near quadrature. In this position the planet has a considerable velocity, either of recession or approach, and any lines due to absorption in its atmosphere would be displaced from analogous lines imposed by the atmosphere of the earth, and thus be made apparent. This ingenious method had been conceived independently by V. M. Slipher, of the Lowell Observatory, and by Campbell. The

[1] *Lowell Obs. Bull.* Nos. 36 and 41.

observations were, in this instance, made at Mount Hamilton. The results of both the 1909 and the 1910 trials were negative, in conformity with those of earlier years, (210) (211) (222).

This evidence did not satisfy the contenders for the existence of life-supporting constituents in the atmosphere of Mars. Finally the inquiry was taken up by Adams and St. John at Mount Wilson. In 1925, after using the powerful permanent installations at the Mount Wilson Observatory, these observers came to the conclusion that the amount of water vapor was 3% of that over Pasadena on the day of observation, and the amount of oxygen was two thirds of that calculated to be above Mt. Everest.[2] These quantities are well within the tolerances indicated in Campbell's observations and would be incapable of supporting life as we know it on the earth. Thus was substantiated, in later years, Campbell's early finding that the amount of water-vapor in the atmosphere of Mars is relatively small. Aside from the purpose for which they were introduced, the incidents related illustrate the difficulty of removing a myth or misconception from scientific literature once it has become effectively lodged therein.

It was with such work as could be done with the limited spectroscopic equipment at his disposal that Campbell occupied himself during his early years at Mount Hamilton until he should have the use of a suitable spectrograph; in fact, as shown by his bibliography, he contributed at that time many papers on subjects outside the field of astronomical spectroscopy. His experience indicated to him the inadequacy of astronomical spectrographs of that day, and he set himself the task of improving them. Means for the construction of a new instrument were generously provided by Mr. D. O. Mills, a sympathetic friend of the Observatory, in the year 1893, and arrangements were made with the optical firm of John A. Brashear to build the spectrograph under general specifications to be provided by Campbell. The latter had the benefit of a very clear and comprehensive statement of optical principles of the spectroscope in a paper then recently published by Keeler,[3] and in the mechanical design of the in-

[2] *Astroph. Jour.*, 63, 137, 1926.

[3] Elementary Principles Governing the Efficiency of Spectroscopes for Astronomical Purposes. *Sidereal Messenger*, 10, 433, 1891.

strument he adopted elements of construction originated both by Keeler and by himself. As is not unusual with instruments of precision, the spectrograph took a long time to build and to test, and it was not until the summer of 1896 that Campbell regarded it as satisfactory (99). Shortly thereafter, his attention was turned in another direction. An expedition to observe the eclipse of the sun of 1898, in India, was projected by Director Holden, and Campbell was entrusted with its organization and conduct. To free him for this duty an assistant was provided to carry on the spectroscopic observations during his absence on the expedition. In October, 1897, shortly before Campbell's departure for India, Director Holden resigned his position, and left the Lick Observatory. Keeler was appointed to succeed him as of July 1, 1898. Although Keeler had, in his earlier engagement at the Lick Observatory, been in charge of spectroscopic work, on assuming the directorship he delegated the responsibility for the 36-inch refractor and the new spectrograph to Campbell, who had just returned from India, and turned his own attention to the Crossley reflector, a relatively new acquisition by the Observatory, which no one else had succeeded in employing to advantage. Keeler overcame certain mechanical defects of the reflector mounting, and began a brilliant series of photographic observations of the nebulae which was most unfortunately terminated by his death on August 12, 1900. Campbell followed Keeler in charge of the Observatory, and was designated director as of January 1, 1901.

During the two years of Keeler's administration Campbell had been busily occupied with the new spectrograph and had made a number of interesting observations, the most outstanding of which was that of the complex nature of the motion of Polaris, which was at the time regarded as indicating that star to be triple, (116). This observation, together with the detection of a large number of spectroscopic binaries, and, generally speaking, the measurement of the radial velocities of stars with unprecedented accuracy, was made possible by the excellent per-

formance of the Mills spectrograph. In fact this instrument, by permitting the attainment of new standards of precision, greatly enlarged a field of astronomical study which had until then been but crudely cultivated. It may be worthy of remark that essentially no new principle, either of optics or engineering design, went into the planning of the spectrograph—one just as good might well have been built in the earliest days of stellar spectroscopy and in all the intervening time been used toward the furtherance of astronomical knowledge. The principles of geometrical optics, which provide a basis for design of most stellar spectrographs, had long been familiar, and the making of stable mechanisms was no new thing.

In view of Campbell's accomplishments, it was but natural that he should be selected to follow Keeler as head of the Lick Observatory. His appointment as director brought to an end the first of the periods into which we have divided his professional life. In it, as has already been said, he did by far the greater part of his work as an observer. A glance at his bibliography will indicate something of his activity in those days; in fact, the bibliography is to be regarded as the corpus of this account, the purpose of the running text being merely to sketch briefly the circumstances which from time to time gave direction to his work. After assuming the administration of the Observatory, Campbell found it necessary to leave the observations very largely to others, though he occasionally took a hand in them. An exception is to be noted in respect to the observation of solar eclipses. He was much interested in these phenomena, and when one was to be observed he was active in every phase of the scientific preparations, and in the conduct of the expedition. This subject will be returned to later.

Campbell's original radial velocity project, regarded in the light of its later development, was modest. As I was at that time his assistant, he naturally spoke to me of his purposes. The more important of these was to issue a catalogue of radial velocities; the second was to calculate, on the basis of this catalogue, the sun's motion among the stars. The possibility that spectroscopic binaries in considerable number might be dis-

covered during the course of the work (there were but a few known at that time) was suggested to him, but he seemed not much impressed. There were about 260 stars on the original program, chosen for their brightness and the measurable quality of the lines in their spectra. They were all north of $-20°$ declination, and, with a few exceptions, were brighter than 5.5 magnitude. They were selected principally from classes F to M, as these spectra are rich in measurable lines. It was expected that each star would be observed four times and that, with luck with the weather, the observations would be completed in about four years. However, as the planned observations approached completion, it was found that those of certain stars on the list had to be repeated (this was especially true in the instances of such stars as had been found to have variable velocities) so that, to fill in the observing time, additional stars were inserted in the list. Finally, to make the work more comprehensive, a completely new list of stars extending in declination $-30°$ was formed. This list contained 830 entries and was eventually succeeded by others too numerous to record, with the result that at this writing, fifty years after the inception of the program, the observations are continuing, and the end is not in sight.

Campbell had realized quite early in his undertaking that in order to complete the catalogue, and to stabilize the solution for the solar motion, it would be desirable to have observations of radial velocities of stars in the southern hemisphere of the sky, and the project of an expedition to obtain them was never very far from his mind. When he became director, he brought this need to the attention of Mr. Mills, and the latter most generously made financial provision for such an undertaking, (151). In 1903 an observatory, with a powerful reflecting telescope and suitable spectrograph, was established on Cerro San Cristóbal, a high hill in the suburbs of Santiago, Chile. The original plan was for an occupancy of about two years, but results secured were of sufficient importance to induce Mr. Mills to provide for the continuance of the work, which he did during the remainder of his life. After his death, in 1910, the project was sustained by his son, and by other friends of the Lick Ob-

servatory. Finally in 1929 the expedition was terminated, and the observatory, with its equipment, was sold to the Catholic University of Santiago, which it is understood still operates it. Campbell planned the observatory, but was never able to go to Chile to see it. It was installed and operated by members of the Lick Observatory staff assigned to those duties.

After the Chilean station got into production, grist came to the hopper from two sources, and the harvest was a rich one. The matter of making the product available to astronomers naturally occupied Campbell's mind. Items of particular interest, such as variable radial velocities, observations of novae, and the like, were promptly published and the quality of the work as well as its scope was indicated. Notwithstanding this liberal disposition, requests, which in instances fell little short of demands, were made that all the information be published promptly. The observatory was in effect accused of retaining data that belonged rightfully to the astronomical public, but there seemed to be no reason to stop in the course of an unfinished program of work in order to issue incomplete determinations of velocity which would eventually require revision. The desire of astronomers to know the radial velocities of as many stars as possible is easy to understand when one recalls that these quantities are fundamental in the calculation of the scale of the stellar system and of the "universe." Most other observations of the stars provide only angular movements, which of themselves supply no knowledge of the absolute (as distinguished from the relative) distances of the stars. The spectroscope, by giving the measure of a star's speed in the line of vision, enables a statistical evaluation of the speed at right angles to that line, which, combined with the observed angular motion, permits the calculation of the average distance of a group of stars. In special instances, for example in the cases of double stars and moving clusters, the distances of individual stars can be derived. Thus interest in the accumulating store of velocity determinations at Mt. Hamilton is readily understood, and Campbell did his best to meet reasonable requests for information. Special observations, or groups of observations were generally available to any responsible person

who asked for them. If Campbell's attitude in this matter requires justification, it is to be found in the fact that radial velocities are subject to systematic errors of various kinds, and these errors can be evaluated only through comprehensive analysis of large groups of observations. One type of systematic error was found to run continuously through the whole series of observations, while another affected only observations made within a certain period of time. Then there is the so-called K-term, which Campbell had found in an early summary of his work, (230) (231) (239). This "term" represents systematic shifts in stellar spectra, dependent upon spectral class, equivalent to velocities, outward from the sun, as high as 4 km. per second. These and other effects had to be isolated and evaluated through a study of all the available material. Had the observations been released in advance of the determination of the corrections necessary to neutralize, or offset these effects, erroneous conclusions would almost certainly have been drawn from the data. The radial velocities of all stars measured at the Lick Observatory, up to January 1, 1927, 2771 in number, were published in catalogue form by Campbell and Moore in 1928, (325). The catalogue is accompanied by a comprehensive discussion of the observations, a redetermination of the elements of the solar motion, and provides a very complete history of the Lick Observatory radial-velocity project. It constitutes the most extensive and homogeneous body of information relative to the radial velocities of stars that has yet appeared.

An account of Campbell's work would be incomplete without some reference to its influence on that of others. Before he began his measurement of the radial velocity of stars such measurements were carried on with only indifferent success at two or three observatories. Following his initial success, a great interest developed in observations of that kind. Campbell's paper: "The Mills Spectrograph of the Lick Observatory," published in 1898, was widely read. His counsel on technical matters was frequently sought, and always freely accorded. Young astronomers trained at the Lick Observatory went elsewhere, and sometimes established themselves in this fertile field.

A very considerable number of the observatories of the world now devote to the measurement of stellar radial velocities a substantial part of their resources, and their combined output of data greatly exceeds that of the Lick Observatory. Probably none will deny to Campbell his due measure of credit for the stimulation of this great development.

When the radial velocity program was first undertaken at Mt. Hamilton, in 1896, about a dozen spectroscopic binaries were known. The orbital motions were, in all these instances, large, and the spectroscopic displacements through which the binary character was revealed were, due to this circumstance, relatively great and had easily been detected with instruments of relatively moderate power. It quite soon appeared from the Lick Observatory observations that stars of this class were more numerous than had been thought, and that the Mills spectrograph, through the attainment of a high degree of precision in measurement, had developed a special field for discovery and study. Further experience confirmed the conjecture that spectroscopic binaries are relatively numerous, and it has been shown that about one star in three has a periodic disturbance of its motion due, undoubtedly in most instances, to the influence of an unseen companion star. It is, however, appropriate to point out that modern research has shown that apparent periodic variable motion in a star can be caused otherwise than by a disturbing companion. In the case of stars that are Cepheid variables a disturbance is believed to be caused by "pulsations" in the star which carry the atmosphere back and forth, so that it is the star's atmosphere which moves, not the star's center of mass. This comment has a definite relation to the sensational discovery made by Campbell in 1900, and already briefly referred to above, that Polaris, the North Star, is multiple. Its velocity was shown to have a compound oscillation, a period of approximately four days being superimposed upon one of several years. (The longer period has since been shown by Moore to be 29.6 years.) It was therefore inferred that Polaris is a triple system, consisting of a bright star revolving about an invisible one once every four days, while this close pair revolves about a distant

star in the course of years, and an announcement was made to that effect. However the discovery was subsequently made by Hertzsprung that Polaris is a Cepheid variable, and it is now believed that the four-day period is the consequence of pulsation in a single star. According to this view the system is a double one, with a period of revolution of 29.6 years.

While Campbell regarded the radial velocity observations and the problems relating to them as constituting his most important work, he was actively interested in other phases of astronomical spectroscopy, perhaps more especially in the spectroscopic study of solar eclipses, as has already been mentioned. One was never quite sure whether he enjoyed more the intense concentration during the critical moments of totality, or the excitement of preparation and travel incident to the undertaking, which sometimes occupied the larger part of a year. To one living on a relatively isolated mountain top, an opportunity to see the world under extraordinarily interesting circumstances is not a negligible matter, but it may be said of Campbell that, whichever of these two aspects of an eclipse expedition appealed to him the more, he served them both well.

Perhaps Campbell's most important eclipse observation was that in confirmation of the work of Eddington and his associates who, at the Brazilian eclipse of 1919, verified the gravitational deviation of light which had been predicted by Einstein from the general theory of relativity. Einstein's prediction of a deflection of star light by the sun's gravitational field, amounting at grazing incidence to 0.″83, based on what has come to be known as the Restricted Theory of Relativity, was made in 1911. Plans to test the prediction at the Russian eclipse of 1914 were made by Campbell and Curtis, but it was a "cloudy eclipse," and nothing came of the attempt. In 1915 Einstein developed the General Theory of Relativity, and from it predicted a deflection of 1.″75. The same observers tried again to detect the deflection, this time at the eclipse of 1918, at Goldendale, Washington, in the northwestern part of the United States. Their equipment was limited, inasmuch as the instruments used in the attempt of 1914 had been held in Russia as a consequence of

the first world war; however, they improvised some cameras and obtained photographs of the star field surrounding the sun. It was hoped that, despite the optical shortcomings of the apparatus used in taking them, these photographs would suffice for the proposed test. The plates were measured and discussed by Curtis who came to the conclusion that they lent no support to the relativity prediction. In the following year, 1919, Eddington and his associates reported, in a communication to the Royal Astronomical Society,[4] that the relativity prediction had been verified by their observations of the Brazilian eclipse in the spring of that year. At the time of this announcement Curtis had left the Lick Observatory to accept the directorship at Allegheny, and Campbell, with the aid of Moore and Trumpler, undertook an examination of his discussion of the 1918 observations. As a result of that inquiry Campbell decided to attempt to measure the relativity deflection at the next favorable opportunity, which would occur at the eclipse of 1922. To this end preparations were made with extraordinary care. The site selected for the observing station was on the west coast of Australia, and there Campbell, with the assistance and collaboration of Trumpler, obtained photographs of a highly satisfactory quality. On the return of the party to California, Campbell was met at the steamer by a delegation from the regents of the University of California, who offered him the presidency of the University. His first disposition was to decline the offer; he was quite satisfied with the office he then held, and wished to continue in it. However he finally yielded to the persuasion of the regents, and accepted their offer, subject to conditions relating to the organization of the University, which were approved by them. On assuming the presidency of the University he retained, nominally, the directorship of the Observatory, without remuneration in that capacity, and Astronomer Robert G. Aitken was designated associate director, with enlarged functions of administration. Campbell was somewhat widely criticised for not relinquishing completely the directorship of the Observatory on becoming president of the University, but a

[4] *Mem.* R.A.S., 62, Appendix, 1920.

careful consideration of the circumstances under which he assumed his new responsibilities will, it is thought, justify his action. The presidency of the University of California is a peculiarly exacting administrative office. Campbell was sixty years of age, with a successful career as a research astronomer behind him, but was without experience of the particular kind required in the management of a large teaching university. He might find the new task uncongenial, or unsuited to his tastes or abilities; and there are always the exigencies of university policy and politics to be considered in such a matter. Indeed the terms of most of the former presidents had been relatively short. That Campbell should undertake the proposed duty was not his own idea but that of the University regents, and there was in the circumstances no reason why, in setting foot to a new path on their solicitation, he should cut off access to the one he had followed with success for more than thirty years. In these circumstances the formal retention of the observatory post would appear to be natural.

With the appointment of Campbell as president, the task of measuring and reducing the plates taken at the 1922 eclipse devolved upon Trumpler. In two papers, authored jointly by Campbell and Trumpler, (315) (322), the results of the investigation are given. There are two sets of observations, made with separate batteries of cameras. The values of the deflection, reduced to the sun's limb, are in one instance 1.″72 and in the other 1.″75. They thus verify the value predicted by Einstein from the General Theory of Relativity; namely, a deflection of 1.″75.

As the record stands, two attempts have been made at the Lick Observatory to confirm the gravitational deflection of light, the first of them, in 1918, gave a so-called negative result; the second, made in 1922, which has just been described, provided a full and complete check of the theory. While there can hardly be a question of the validity of the second result, the situation has its awkward aspects, and it is to be hoped that eventually the earlier attempt at confirmation will be reviewed by a qualified person.

Campbell's administration of the University was most successful, as the following quotation from an appreciation of his life, prepared by a committee of the Academic Senate of the University, testifies: [5]

> "For six months before he entered upon his duties as President, he devoted himself to the University's history, organization, functions, and problems—all with his usual thoroughness. During this period he sought the advice of leading men of the factulty. As a result he entered upon his duties as President with a remarkable grasp of the University's functions and needs.
>
> "His devotion to truth and his belief in using the methods of science were evidenced in all his acts and policies. 'The fundamental purpose of universities,' he announced in his inaugural address, 'is to hasten the day when all men and all women shall have comprehension of the truth, so that they may live their lives more richly and more usefully in this exceedingly interesting world.' 'The first . . . obligation of a university,' he continued, 'is to instruct the students who come knocking at its doors; to disseminate . . . the knowledge that has been gained and preserved in all past time.' 'The second great function of a modern university is to extend the frontiers of knowledge into regions as yet unexplored.' Every professor, in his opinion, faced the duty of doing something to add to our store of human knowledge.
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> "The Academic Senate closed an address to President Campbell concerning his retirement with this characterization: 'Your administration has been a period of tranquillity and healthy growth such as few universities have enjoyed, and we, the Academic Senate, desire to express to you our heartfelt appreciation.' Dr. Campbell gave himself wholeheartedly to the duties of the presidency, mindful of the good of the institution. His policies harmonized with the truth; his decisions seldom required revision. Of his conduct of the presidency, Regent Chester Rowell has said: 'With a hand always gentle but always firm and never shirking, President Campbell ruled the University wisely and well. Whether in its nominally ruling board, in its faculty, or in its student body, there are problems great and small in every university. The great ones, Dr. Campbell faced greatly, seeing them in the full perspective of the long

[5] *In Memorium*, 1938, The University of California, pp. 5-6.

future and of their wide ramifications, as was natural to a scientist whose habitual intellectual background had been the whole harmony of the universe.' "

Campbell was retired in 1930 from the presidency of the University and the directorship of the Observatory. In recognition of his long and excellent service the regents of the University invited him to continue indefinitely in the occupancy of the residence at Mount Hamilton that he had enjoyed for so many years, and he made more or less tentative plans for the resumption of scientific work. He was, however, in failing health, and had in the later days of his residence in Berkeley lost the sight of an eye. Furthermore he had been occupied almost exclusively with administrative matters during the preceding eight years, and in that time developments in astronomical spectroscopy, the field of his special interest, had been prodigious. I recall one day standing near him in the small mountain post-office where the members of the community customarily gather at noon to receive their mail, and word from the world. He opened a letter, read it once and again, then handed it to me with the question "What do you think I ought to do about this?" It was a note from the nominating committee of the National Academy of Sciences asking whether he would accept the nomination to the presidency of that body, which nomination is virtually equivalent to election. In view of the circumstances that have just been sketched, and not realizing the magnitude of the task to which he would set himself, I urged that he accept, which after some deliberation he did.

Campbell assumed the presidency of the National Academy on July 1, 1931. He was deeply sensible of the responsibilities imposed by the office, and, in order that he might be free to discharge them, he and Mrs. Campbell established a home in Washington, where they lived for the greater part of his term of office. Campbell devoted to his new position the same particular and minute attention that he was accustomed to accord to whatever he had to do, and the business affairs and general routine of the Academy were well ordered; but it will probably be agreed that the outstanding feature of his administration was the zeal with

which he sought to uphold the prestige of the Academy, especially by strengthening its position as adviser on scientific matters to the Government of the United States.

It will be recalled that the Academy was created by the Congress in 1863, during the American Civil War, through an act of incorporation which specified no duties other than that the Academy should hold an annual meeting and should "whenever called upon by any department of the Government, investigate, examine, experiment, and report upon any subject of science or art . . ." For many years following the establishment of the Academy the Government sought and received from it advice upon important matters, but in the decade or more preceding Campbell's term as president there developed, perhaps in part inadvertently, a tendency to delegate advisory functions to newly created bodies. It was Campbell's view that the act of incorporation, reinforced by the record of the Government's practice in seeking the Academy's advice, established the Academy as presumptively the advisory agency to the Government on scientific matters, and he sought by all means in his power to keep open the channel through which such advice might flow. "With tremendous courage and persistence, irrespective of personal considerations, he succeeded in obtaining fullest recognition of the Academy by the President (of the United States) through the creation within the Academy, on invitation of the President, of the Government Relations and Science Advisory Committee."[6] In the words of a qualified and eminently fair commentator upon Academy affairs of that period, whose views on method and procedure were not uniformly consonant to those of Campbell, "Campbell had a high and correct view of what the Academy is and should be, and did much to put it in its proper position in the framework of government."

On the conclusion of Campbell's term of office he and Mrs. Campbell returned to California, and after a short stay on Mount Hamilton made their home in San Francisco. They had a beautiful apartment overlooking the blue waters of the Golden Gate and the mountains beyond, where their many friends were wel-

[6] loc. cit. p. 7.

comed and hospitably entertained. Although he did not actively participate in scientific affairs at the time, Campbell took a general interest in current developments and viewed the astronomical scene with intelligence and satisfaction. He was in his middle seventies, and in failing health, though he seldom referred to his infirmities, and never permitted them to mar the pleasure of his guests; nevertheless they grew, and, in particular, he found himself losing the sight of his remaining eye. Without cause, other perhaps than that his memory was, in unimportant respects, not so good as it had been, he feared impairment of his reason, and was fearful lest he become a burden upon others; eventually his worry could not escape the observation of his friends, and it became to them a matter of concern. However, the last time I called upon him he was seemingly better than for a long time; he was bright and cheerful, with apparently not a care in the world. A few weeks later, on June 14, 1938, his friends, and the community, were shocked by his tragic death.

One who attempts to appraise the traits of character and other factors that contributed to Campbell's success must be struck by his extraordinary capacity for hard work. While he appreciated the value of relaxation and enjoyed active recreation, one does not recall ever to have seen him doing nothing, that is to say if he could help it. If there were ten minutes to spare, he could usually contrive to find some way of using them to advantage. A quality perhaps related to industry is perseverance, and he certainly had the capacity, once he had set his hand to a task, to sit tight and see it through. A factor contributing to the fullness of Campbell's career, second in potency to none, was the serenity of his domestic life. He was married to Elizabeth Ballard Thompson on December 28, 1892, shortly after he had established himself at the Lick Observatory. Mrs. Campbell and their three sons, Wallace, Douglas, and Kenneth, survive him. Mrs. Campbell was an ideal companion to her husband. She presided with dignity and charm over their home, and accompanied him on his many travels, even on the journey that led so far afield as the desolate "forty-mile beach" of western Australia, the site of the 1922 eclipse station. She served on all

of his eclipse expeditions in the important capacity of chief of commisariat, and had the health and well being of her colleagues in her keeping; also she found time to join them in their scientific observations. Mrs. Campbell was and is a person of rich wisdom and unhurried judgments. That her counsel profoundly influenced the course of her husband's life none who had the privilege of knowing them will be likely to question. In commenting upon a life of great accomplishment one cannot but reflect upon a personality that provided probably its greatest inspiration.

Campbell was the recipient of many honors, and occupied various posts of distinction and responsibility. Among them are the following:

Trustee of the Carnegie Institution of Washington.

Lectureships:

Silliman Lecturer, Yale University, 1909-10; William Ellery Hale Lecturer, National Academy of Sciences, 1914; Halley Lecturer, Oxford University, 1925.

Honorary degrees:

M.S. University of Michigan, 1899.
Sc. D. University of Western Pennsylvania, 1900; University of Michigan, 1905; University of Western Australia, 1922; Cambridge University, 1925; Columbia University, 1928; University of Chicago, 1931.
LL.D. University of Wisconsin, 1902

Membership in scientific societies and organizations:

National Academy of Sciences, President 1931-35; American Philosophical Society, Vice President 1924-30; International Astronomical Union, President 1922-25; American Academy of Arts and Sciences; American Association for the Advancement of Science, President 1915; American Astronomical Society, President 1922-25; Die Astronomische Geselleschaft: Astronomical Society of the Pacific, President 1895 and 1910; Seismological Society of America.

Honorary membership in the following academies and organizations:

Royal Astronomical Society, London; Royal Academy of Sciences, Stockholm; Royal Academy of Sciences, Upsala; Society of Italian Spectroscopists, Rome; Royal Society of London; Madrid Academy of Sciences; Royal Society of Edinburgh; Russian Astronomical Society, Moscow; California Academy of Sciences, San Francisco; Royal Institution, London; Royal Astronomical Society of Canada; Institut de France (Paris Academy of Sciences); Bureau do Longitudes, Paris; Russian National Academy, Leningrad; Royal Italian Academy of Sciences (dei Lincei), Rome.

Decorations:

Commander of the Order of Leopold II, with gold insignia, Belgium, 1919; Officer of the Legion of Honor, with gold insignia, France, 1926; Commander of the Order of the Crown of Italy, with gold insignia, 1929.

Medals:

Paris Academy of Sciences, Lalande Medal, 1903; Royal Astronomical Society, London, Annual Medal, 1906; National Academy of Sciences, Washington, Draper Medal, 1906; Paris Academy of Sciences, Janssen Medal, 1910; Astronomical Society of the Pacific, Bruce Medal, 1915.

Sources of information: A scientific and personal association with Doctor Campbell of many years duration. This has been supplemented by a brief biographical sketch prepared by Doctor Campbell, and by a few letters from Mrs. Campbell. The *bibliography* was compiled by Doctor Campbell, and has been only lightly edited.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Astr. Nach. = Astronomische Nachrichten
Astron. and Astroph. = Astronomy and Astrophysics
Astron. Jour. = Astronomical Journal
Astroph. Jour. = Astrophysical Journal
L. O. Bull. = Lick Observatory Bulletin
Mem. Nat. Acad. Sci. = Memoirs, National Academy of Sciences
Pop. Astron. = Popular Astronomy
Pop. Sci. Mo. = Popular Science Monthly
Publ. L. O. = Publications of the Lick Observatory
Publ. A. S. P. = Publications of the Astronomical Society of the Pacific

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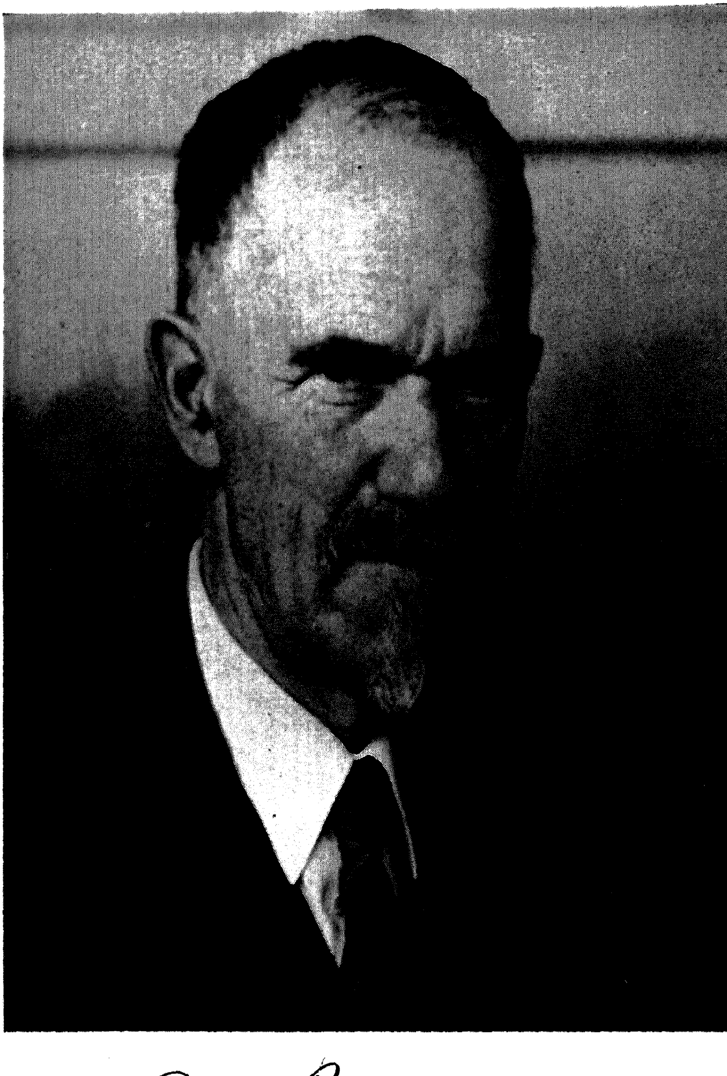

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CHARLES BENEDICT DAVENPORT

1866–1944

BY

OSCAR RIDDLE

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1947

CHARLES BENEDICT DAVENPORT[1]

1866–1944

BY OSCAR RIDDLE

Charles Benedict Davenport, distinguished zoologist, geneticist and eugenist, was born on his father's farm near Stamford, Conn., on June 1, 1866. This farm, during six months of spring and summer, was occupied by the Davenport family although their more permanent home and the father's business were located in Brooklyn.

The father, Amzi Benedict Davenport, was of Puritan stock. He published in two editions (1850 and 1876) an elaborate genealogy of the Davenport family that went back continuously to 1086. Amzi grew up as a farmer's boy at the home near Stamford, but before he was 20 he became a school teacher and established a private academy in Brooklyn in which he taught for 16 years. In 1853 he set up a real estate office, dealt in insurance and managed estates, acquiring a high reputation for honesty and reliability. He married twice and was the father of eleven children. He was deeply religious in the Puritan manner, and his attitude toward his children was exceptionally harsh and unyielding. Though very energetic, with a brisk walk, he was nervous, astigmatic, partially color blind and, from 1880 to his death from pneumonia (following breaking of leg) in 1894, had chronic rheumatism or arthritis.

The mother, Jane Joralemon Dimon, was of English-Dutch-Italian ancestry. She held liberal religious views, was interested in natural history, and extended warm affection and encouragement to collegiate education to her children. Her father, John Dimon, a farmer's son and carpenter from East Hampton, Long Island, became an active citizen of Brooklyn, where he served as Commissioner of the Alms House and, for several years, as Alderman. Her maternal grandfather, Teunis Jorale-

[1] The biography published by MacDowell has been drawn upon freely in the preparation of the present Memoir: Charles Benedict Davenport, A study of conflicting influences, by E. Carleton MacDowell, *Bios*, vol. 17, No. 1, 1946.

mon, son of a Dutch farmer in New Jersey, acquired a farm on Brooklyn Heights and became village trustee, and later a judge, in Brooklyn. He accumulated a considerable fortune, parts of which gave financial independence to his grandaughter. Charles was the eighth child and last son of Amzi and Jane. An older brother, William Edwards Davenport, became an ordained minister and social worker in Brooklyn. A younger sister, Frances Gardiner Davenport, attained success in historical research.

The early education and youth of Charles transpired under conditions which, at the time, were quite unusual in America. Except for the winter of 1874-1875 he did not attend school until near the age of fourteen, but was taught by his father, for whom, it appears, he concurrently performed endless chores at the real estate office and on the farm. Besides writing other things Charles early wrote a diary from which we learn that in winter he was office boy and janitor of the office, which he opened, swept and dusted every morning; there he did errands to change "For Rent" signs, frequently collected rent bills, went to the tax office, and studied his lessons.

During two years, we are told, these lessons consisted of "doing sums and stuiding Smith's Grammer." The father secured these services for 25 cents per week and, of course, the lessons he gave when he had time. Frequently this was in the evening while he clipped papers. If Charles failed he had to go to bed at once. The same penalty was paid for not knowing his Sunday School lesson, which was heard by Amzi even more regularly than other lessons. At home Charles was handyman, blacking his father's boots, carrying coal and ashes, and shovelling snow. He was stable boy also when the carriage horse was in town. He had often accomplished much before he reached the office about 8:00 A. M. His diary states: "O! I want to go to school. I hate to be in the office—that Prison House, as I call it." His first eighteen summers were spent on his father's farm. He early became a regular farm hand, tended stock, worked in the fields, and drove to and from the station with his father, who made frequent trips to Brooklyn.

Although busy with farm work his lessons were continued, and these summers gave splendid opportunity to watch bird migrations, collect insects, and profit by his mother's knowledge of natural history. Thus was established the interest that determined his life work.

When nearly fourteen (November 26, 1879) Charles was permitted to attend school. He then entered the Brooklyn Polytechnic Institute, where his previous isolation and early responsibilities marked him with premature seriousness and independence. To the end of his days he was to remain essentially a lone man, living a life of his own in the midst of others, and feeling somewhat out of place in almost any crowd. Spontaneous or organized group games or athletics seem to have been unknown to him at any age. His informal preparation was, for a time, a handicap at the Polytechnic, but this seems to have intensified the spirit of competition called out by contact with his equals. He excelled in history, mathematics, composition and the small offerings in natural history. After four years, corresponding to the end of high school, he was at the head of his class and gave the commencement oration. Charles continued his work, largely in civil engineering, at the Polytechnic Institute, from which he received the B.S. degree in 1886.

From an early age Charles was a writer and organizer or participant in informal, juvenile groups interested in natural history. At about 9 or 10 years he was secretary of such a society (Excelsior) which maintained a museum in a room on the top floor of his grandfather's house, and when 11 to 13 years old he edited "The Twinkling Star," an amateur monthly. Later (1881) he was vice-president of a short-lived Brooklyn Chapter of the Agassiz Association. Throughout his last year in the Institute, Charles was editor-in-chief of the full-fledged school monthly, *The Polytechnic*. During the summer of his eighteenth birthday he spent two months as field reporter on the *White Mountain Echo*, of Bethlehem, N. H. At this and even somewhat later periods his requests or discussions of personal matters with his father usually assumed the form of long, or quite long, letters. Replies often assumed the same form, though

father and son continued to spend considerable time in the same office.

For nine months (1886-1887) following his graduation Charles was joined, as a rodman, to a group in northern Michigan engaged in a survey of the Duluth, South Shore and Atlantic Railroad. The latter half of this period was also utilized to acquire sufficient Latin to provide admission to Harvard. His classmate and close friend, Herbert H. Field, later founder of the Concilium Bibliographicum in Zürich, had taken the liberal arts course at the Polytechnic and was thus prepared to enter Harvard the previous autumn. Field strongly urged Charles to come to Harvard. These conditions and events led Charles to break from engineering, and indeed from the domination of his father, and try for college, a goal for which his training had not fully prepared him.

With his mother's backing, tutoring, and a job in the Division of Water Work, Massachusetts State Board of Health, he obtained funds for the first frugal student years. Later the university provided various offices, culminating in an instructorship (1893-1899). He received an A.B. from Harvard in 1889 and a Ph.D. in 1892. Here Charles, as student, responded by an enduring devotion to his inspiring teacher, Prof. E. L. Mark. As instructor, he taught both undergraduate and graduate courses, the latter with notable success; and he investigated zealously and wrote prolifically. During the ten years, 1890-1899, he wrote twenty-five scientific papers and four books. The two volumes (1897, 1899) on "Experimental Morphology" served to stimulate the movement, already in progress in Europe and America, to apply experimental methods to zoological and embryological materials. His first book (1893), on "Graduate Courses—a Handbook for Graduate Courses," was probably of temporary and local interest only; its size and place of publication are unknown to the present writer. "Statistical Methods With Special Reference to Biological Variation" (1899) was the first book to bring the newer investigations of Karl Pearson to popular attention in the United States.

Davenport gave certain courses in the "Annex," later called

Radcliffe College. Here he met Gertrude Crotty, a graduate student who had been an instructor in zoology at the University of Kansas. They were married on June 23, 1894. After the infancy of their two daughters she took, during several years, an active part in his work and was co-author of several papers. Following their marriage she, and she alone, was Davenport's confidante and chief counselor. The mother of a growing family was keenly aware of the advantages of a growing income which the prolonged instructorship at Harvard did not provide. Week by week during these years she turned to the death notices in *Science* to learn of a position that might be open. Davenport became, and remained, highly conscious of the value of money and devoted to a good bargain. Advancement came with his appointment as Director of the Summer School of the Biological Laboratory of the Brooklyn Institute of Arts and Sciences, Cold Spring Harbor (1898-1923). In September of the following year (1899) he accepted an assistant professorship at the University of Chicago where he was promoted to an associate professorship in 1901. In 1904 he resigned the Chicago post and became Director of Carnegie Institution's newly established Station for Experimental Evolution (called Department of Genetics after 1921) at Cold Spring Harbor, N. Y.

During the later years at Harvard, Davenport published several researches with his students as co-authors: acclimatization to high temperatures (with W. E. Castle) and to poisonous chemicals (with H. V. Neal); heliotaxis (with W. B. Cannon); geotaxis (with H. Perkins); phototaxis (with F. T. Lewis) and comparative variability (with C. Bullard). Just prior to going to Chicago he wrote (1899) that by using modern quantitative methods as the key to the relation between specific form and geographical distribution, he hoped to throw light on the origin of species out-of-doors. Such studies, however, led almost inevitably to ecology. Though Davenport published only one strictly ecological paper, "The Animal Ecology of the Cold Spring Harbor Sand Spit," his interest and influence were so effective at that time that animal ecologists regard him as one

of their pioneers. Among his students at Chicago were C. C. Adams and V. E. Shelford. In this effort at Chicago, Davenport had the active support and cooperation of Whitman and Coulter. It was at Chicago, moreover, where plans had long been developing for a new kind of institution—a biological farm, equipped to carry on prolonged and uninterrupted studies of heredity, variation and related subjects. Prof. Charles O. Whitman of that University and director of the Marine Biological Laboratory at Woods Hole since its beginning, presented plans in detail before a meeting of the American Naturalists in December 1897, and emphasized the many mutual advantages that would result from its location near the Marine Biological Laboratory at Woods Hole. At this same meeting Davenport also spoke of the needs of a farm, or zoological preserve, for the study of phylogenetic problems.

Immediately succeeding events have been well described by MacDowell:

"Whitman knew very well Davenport's interest in evolution and, before the end of the first season at Cold Spring Harbor (1898), Davenport was tendered an invitation to take charge of the department of beginning investigation at Woods Hole the next year. This was declined on the basis of 'the inexpensiveness and pleasures of the Cold Spring Harbor summer'; his 'mercenary motives,' he wrote. Cooperation, even on a national scale, had no attraction for the man who was having his first experience as first in command. Whitman, however, persisted; if not Woods Hole, then an assistant professorship in his department at Chicago. The offer was received in September 1899 and was accepted. And so Davenport was brought into intimate contact with the most active discussion in the country of plans for a new institution to study evolution. To be sure, the details were all fitted to Woods Hole.

"Cold Spring Harbor, however, had a summer school and highly varied habitats and the proximity to New York might weigh heavily against Woods Hole's greater isolation. Moreover, at the end of 1901, the future of the Laboratory at Cold Spring Harbor was uncertain. There was talk of transferring its control from the Brooklyn Institute of Arts and Sciences to Columbia University, with an inevitable change of director. If the Brooklyn Institute should expand the Laboratory by the establishment of a permanent resident staff to study evolution,

this change could be forestalled. The associate professorship was an advance in status, but financially it was still inadequate. The Directorship of a new laboratory would mean a real advance and give tremendous opportunities to organize and map plans; no more classes or university routines, no more worry about recognition or advancement; to do as he pleased and shoot ahead as fast as he desired—nothing could be more alluring.

"The Carnegie Institution of Washington was incorporated on January 4, 1902. Davenport's first communication to this Institution was dated January 16, 1902, and was delivered by hand to the secretary, Dr. Charles D. Walcott, by Mr. Charles L. Hutchinson, vice-president of the Corn Exchange Bank of Chicago, who went to Washington to attend the first meeting of the Institution's Board of Trustees. This was the opening move of a two-year campaign whose final success gave Davenport a position of extraordinary influence and power, and gave his name a lasting place in the history of science.

"That there was such a campaign may seem surprising, but the lengths to which it was carried would be unimaginable without the original documents in the archives of the Carnegie Institution, in Washington. For once, the major influences and urges of his life worked in the same direction and their unified pressure became excessive. Davenport's procedures in this critical period have the greatest importance for an understanding of the man. The pressure was possibly never again so great; but on subsequent occasions, though less well documented, it is possible to recognize repeated use of the methods seemingly approved by success. In the present period the incandescence of his enthusiasm distorted his judgment and permitted exaggeration that bordered closely upon misrepresentation.

"The application was repeatedly submitted to the Institution in different restatements; the financial requirements were progressively reduced, as by the Biological Laboratory's offer of free land and, finally, by Davenport's proposal to raise funds for a building from other sources. One of these applications, dated May 5, 1902, was sent directly to the Carnegie Trustees and published in the first year book of the Institution."

The Davenports spent four months in Europe in the autumn of 1902 collecting *Pecten* shells for statistical studies of geographical variation, and visiting many marine laboratories from Bergen to Naples. It was on their return to Chicago at the end of that year that the writer first met them. Though I took no courses with Davenport, I could speak with him, first of all, of

his earlier letters requesting me to collect *Pecten* for him in Puerto Rico, and saw him often at the weekly departmental "Seminars." Twelve years later I became a member of the staff which he brought together at the Department of Experimental Evolution. This association lasted 20 years; and thereafter, during 10 years of his retirement, we were neighbors and associates. Davenport's letters to the Carnegie Institution continued in volume until October 1903, when he got unofficial word of his prospective appointment as director of a Station for Experimental Evolution to be formed by Carnegie Institution at Cold Spring Harbor. The official appointment was made on January 19, 1904, when Davenport was already in Cold Spring Harbor. Of this period MacDowell writes:

"The position was secure, but the program was as ill-defined as it had always been, and as it was to remain. The issues of the campaign had been geographic and personal; the specific experiments that were to solve the problems of evolution had been subordinated as relatively unimportant details. Varying lists of experiments had been proposed, but the differences in the successive lists did not represent progressive critical thought. So a laboratory was established with a staff and a building—but without a well-planned program. In March, the director wrote, 'I have little notion of just what we shall do. We shall reconnoiter the first year. . . . My own work will be largely a reconnaissance of capacity for maintaining, breeding and crossing wild animals in captivity and also the study of the behavior of unit characters in hybridization of domestic races of birds.' . . .

"In the early years of the new laboratory, Davenport personally undertook breeding experiments with snails, mice, house flies, moths, sow bugs, trout and cats; but publishable results were not obtained, owing, in most cases, to difficulties with breeding techniques. Canaries and chickens, however, did breed and provided the basis for four beautifully illustrated publications. These, with a series of papers with E. G. Ritzman on sheep, constitute his major experimental contributions to genetics. There were, besides, a large number of brief notes, annual reports, reviews and addresses on animal genetics. The chicken papers represented a real advance over the quality of the previous work, although the meticulous oriental accuracy of the illustrations, painted by Morita, was missing from the

tables. The canary paper gives shocking evidence of speed too great either for consistent tables or for sound logic. Such speed, with such effects, became habitual."

From 1900 to 1904 Davenport had bred mice of all the basic colors without finding a genetic interpretation; he accordingly concluded that there were unquestionably broader principles of heredity than those discovered by Mendel. He continued to hold this doubting attitude toward Mendelism until the supporting evidence began to appear from all sides and Bateson visited him. But as soon as the general acceptance of Mendelism was apparent, he became a staunch supporter and proceeded to make human applications.

Soon after Davenport's appointment as director of the new laboratory Prof. E. B. Wilson, of Columbia University, was appointed a special adviser of the Carnegie Institution on the organization and work of the laboratories then being established at Cold Spring Harbor and Dry Tortugas. In this capacity Wilson wrote a letter to Davenport suggesting a conference. This letter was curtly dismissed as "interference," and it seems that Wilson made no further attempt to fulfill his mission with respect to this laboratory.

With Gertrude C. Davenport as senior author a series of papers began to appear in 1907 on human heredity—color of eye, skin and hair, and hair form. At this time only one of his four publications on avian genetics had appeared in print. Concerning this shift of his interests Davenport wrote (Annual Report, Carnegie Institution, 1909): "Although not strictly within the scope of experimental work the necessity of applying the new knowledge (laws of heredity) to human affairs has been too evident to permit us to overlook it." Thus began the active interest in eugenics which was soon to terminate his participation in genetic experiments, but which made him the leading exponent of eugenics in America.

In 1910 Davenport succeeded in persuading Mrs. E. H. Harriman to provide funds for the establishment of the Eugenics Record Office at Cold Spring Harbor. Ultimately her donations totaled considerably more than half a million dollars. In 1918

a simplification of administrative matters was brought about by the Carnegie Institution in accepting from Mrs. Harriman the ownership of the Eugenics Record Office. The work of the Record Office was maintained until about 1940, and thereafter abandoned. In the administration of the Eugenics Record Office, Davenport was assisted by Dr. H. H. Laughlin as superintendent until 1921 (when this Office was combined with the Station for Experimental Evolution to form the Department of Genetics) and thereafter as assistant director.

As a zoologist and geneticist Davenport knew, and greatly aided in the dissemination of the doctrine, that the germ cells do not belong to a person in quite the same way as does his hair or his stomach; that the way a person reacts to a given stimulation is determined by the germinal determinants that have fallen to his lot and to the training and experience that have favored or repressed the complete development and fruition of such determinants; that men are genetically unequal; that medicine and philanthropy tend to preserve the biologically unfit; and that race mixture, unselected immigrants, and unequal rates of reproduction in various native groups, all affect the future welfare of our nation. Indeed, such factors are of basic concern to the human race.

The Davenport eugenics creed, in abbreviated form, is given herewith:

"I believe in striving to raise the human race to the highest plane of social organization, of cooperative work and of effective endeavor.

"I believe that I am the trustee of the germ plasm that I carry, that this has been passed on to me through thousands of generations before me; and that I betray the trust if (that germ plasm being good) I so act as to jeopardize it, with its excellent possibilities, or, from motives of personal convenience, to unduly limit offspring.

"I believe that, having made our choice in marriage carefully, we, the married pair, should seek to have 4 to 6 children in order that our carefully selected germ plasm shall be reproduced in adequate degree and that this preferred stock shall not be swamped by that less carefully selected.

"I believe in such a selection of immigrants as shall not tend

to adulterate our national germ plasm with socially unfit traits.

"I believe in repressing my instincts when to follow them would injure the next generation."

The personal characteristics of Davenport, the mature man and investigator, have been made partly apparent in the preceding pages. Earlier and later ventures and accomplishments in his career may be better understood if some further statement is made concerning his personality. Davenport worked with great independence and with intense application. Only about 10 percent of his total of approximately 439 publications (including abstracts, etc.) were written in collaboration with others. G. H. Parker relates that while in the Harvard Laboratory, Davenport wore, as he bent over his microscope, a large eye-shield on which was inscribed "I am deaf, dumb, and blind." This effort to exclude all external disturbance during his work continued during his entire life. He was often writing in his office at 6:00 A. M. He daily took long walks, using a rapid stride; and walks of five, ten, and even fifteen miles were sometimes taken to catch trains, attend meetings, or do errands. Throughout life he maintained a boyish eagerness and enthusiasm. Though lean and nearly six feet tall he showed little or no evidence of the dyspepsia which, in some degree, he had throughout most of his life. He was practically devoid of manual skill, except at drawing (illustration of his own early scientific papers). He was shy, and as MacDowell concludes, consciously or unconsciously contended against marks left by his boyhood repression and sense of inferiority. He had an insistent urge to write, and some of his scientific work suffered from being written before it was properly prepared and digested. He freely and effectively gave of his time to young biologists who sought his aid. He did not seek, nor often accept, the advice of distinguished biologists or his staff concerning policies of high importance. At his home he was charming and hospitable, ready to converse on one or more of many things uppermost in his mind on his walks, or on topics of interest to his guests.

Several published criticisms, sometimes severe, of parts of Davenport's work in avian genetics, eugenics and in anthropology

are very difficult to refute. Nevertheless it must be admitted that the feverish and multiform activity which characterized Davenport's life resulted in large gains to the several areas of zoology, biometry, ecology, genetics, eugenics and physical anthropology. This is an impressive reward for a lifetime of unflagging effort. Davenport was unquestionably one of the leaders of biology in his generation; and his generation was one in which biology made phenomenal advances. It is almost certain that this leadership resulted mainly from his abilities as a promoter and organizer and less from his abilities as a scientific investigator. Administrative posts certainly aided him in the attainment of eminence, but from the standpoint of most of his superiors and subordinates he lacked the skills and traits of an administrator. His was an unusual personality, and he was peculiarly unable to estimate or sense the personalities of others. He recorded himself as of "nervous" temperament. In his promotional efforts, even where apparently or temporarily successful, it is probable that science was sometimes ultimately the loser; a lack of balance in Davenport's several abilities markedly limited the magnitude of his total contribution to science. Yet he achieved much more than personal success; he expanded several sides of his many-sided science.

In 1909, and several times later, Davenport again spent several weeks in Europe. At the outbreak of World War I he was attending a meeting of the British Association in Australia; this trip involved short stops at islands of the Pacific where, he noted, the native population stimulated his interest in anthropology. Later trips were made to Yucatan, Jamaica, and parts of Canada, all in pursuit of specific problems in race-crossing, eugenics and anthropology.

Man remained Davenport's subject from 1907 for the rest of his life, excepting a brief return to experimental work in 1925 and 1927 (mouse endocrines).[2] The publication of "Heredity in Relation to Eugenics," in 1911, presented evidence on a wide range of human traits. While this was impressive,

[2] Immediately succeeding statements are taken with little change from MacDowell's biographical study.

its continued usefulness was reduced by its hasty preparation and the lack of critical judgment in lumping together, indiscriminately, cases with ample and with insignificant evidence. The topics of Davenport's special studies were likewise highly diverse. Besides the more familiar subjects of stature, body build and longevity, he investigated goiter, otosclerosis, neurofibromatosis, pellagra, epilepsy, mental disorders, temperament, mental attributes of naval officers, mongoloid dwarfs, twinning, sex-linkage, and race crossing. The implications of eugenics for state, church, medicine, and society in general were discussed wherever he could find a platform and an audience. His scientific background and associations gave him the prestige of an authority in the eyes of those inclined to accept his position on the social and political aspects of eugenics. But the opposition of many, instead of quickening the search for more accurate and convincing evidence, called forth a defensive attitude which led to exaggerated emphasis and dulled objective thinking.

Through the initiative of a Committee on Anthropology of the National Academy of Sciences, Davenport was commissioned (1918) as a major in the Sanitary Corps and assigned to the Surgeon General's Office to summarize the physical records of recruits, with Lt. Col. Albert G. Love, M.D. Working through the greater part of the year in Washington, with frequent visits to Cold Spring Harbor to keep things going, Davenport cooperated in the preparation of four volumes largely filled with tables and graphs showing the frequency of the different conditions and defects recorded and their geographical distribution. This work gave an anthropometrical trend to Davenport's interests that grew and occupied more and more of his time, especially after retirement. By measuring institutionalized children as they grew, notably those at Letchworth Village, N. Y., he collected data for extensive studies on growth curves and changes in proportions accompanying growth. With the hope of creating renewed interest in child development, he published, in 1936, "How We Came By Our Bodies," a popular book of 401 pages and 236 illustrations describing, from the stand-

point of current sciences, the course of development and the role and origin of genes.

At the time the work on army records began, Davenport had been directing simultaneously three institutions at Cold Spring Harbor for a period of eight years. During this time the growing concentration of his interest in eugenics did not prevent the material expansion of all three laboratories. The Eugenics Record Office had acquired a new building; so had the Station for Experimental Evolution, as well as an enlarged staff; the Biological Laboratory gained an endowment fund. The association of the three laboratories seemed to offer great strength; each had its own field and each had much to gain from the others. The one director for all seemed to guarantee effective cooperation. The vision and the goal were excellent, but the requirements for realization were not recognized. To secure support, put up buildings, and engage workers is not enough. There remained the need of fostering the integration of the work of highly individualistic investigators, in itself a biological problem of the highest order of complexity. Further, the successful administration of three adjacent laboratories with differing financial status, and governing boards with different points of view, called for extraordinary scrupulousness, tact, and understanding to avoid pitfalls. Instead of integration within and between the three laboratories, difficulties continually arose that blocked effective cooperation. Davenport gave his time and energy to the limit, but the days were not long enough to satisfy all the claims on his attention; and every move involved a choice, deliberate or not, between competing loyalties—loyalties to different institutions, to staffs, to family, to friends, to innumerable outside interests, to his early training, to his scientific ideals, his ethics, his objectives.

There was no time for, and, indeed, seemed to be little interest in, the deeper general significance of even his own studies. Thus, for the groups of scientists he had assembled he did not provide leadership on a philosophical plane. He did not inspire the mutual confidence on which the most effective operation of a group depends. He was criticised at home and abroad, but he

could not accept criticism objectively. His intolerance of criticism was frankly enough admitted by himself, but in the presence of those who might criticise, he became increasingly silent, constrained, and ill at ease. This applied to many colleagues, and especially to those of his own strong staff at the Station for Experimental Evolution, in whom an insidious atmosphere of apprehension was developed by his practice of saying little and announcing plans by action.

At the urge of Carnegie Institution, Davenport, still partly engaged in the Surgeon General's Office, resigned from the directorship of the Biological Laboratory (1923). This and the earlier consolidation of the other two laboratories at Cold Spring Harbor failed, however, to concentrate Davenport's activities. Following this retirement he accepted even broader responsibilities. At precisely this time the Brooklyn Institute of Arts and Sciences wished to relinquish its control of the Biological Laboratory; and so Davenport planned, solicited memberships and funds, and incorporated the Long Island Biological Association to assume ownership of the laboratory. Officially he was only its secretary, but he remained its leading spirit.

In discharging his duties as director of the Biological Laboratory, Davenport gathered around him each summer a body of enthusiastic young workers whose biological growth was shaped, as noted by G. H. Parker, by his example rather than his precepts. These workers were to him as apprentices to a master. In this way Davenport exerted during twenty-five years a generous and salutary influence over many growing biologists. These new recruits not only became interested in Davenport's field of research, but often added materially to the solution of its problems. In later years he attempted, with slight or partial success, to establish some all-year-round researches in that institution. Still later, and after summer classes had been abandoned, he assisted in arranging for each summer a short conference of specialists on some live biological topic, and for publication of the addresses and discussions in a volume of a now thriving series known as Cold Spring Harbor Symposia on Quantitative Biology.

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Fascinated by organization since boyhood, Davenport never stopped organizing. Besides the Long Island Biological Association, he was a founder of the Galton Society, the Aristogenic Association, the Eugenic Research Association, the Tax Payers League, the Cold Spring Harbor Whaling Museum. At Chicago he was involved in an attempt to form a Western Branch of the American Society of Naturalists. Membership in a society usually meant action; it might mean committee work, collection of funds, or reorganization. He held many offices, including the presidency or vice-presidency of ten societies. MacDowell has classified his 64 memberships as follows: natural history (6), zoology (5), genetics (2), eugenics (7), anthropology (5), medicine (5), general science (11, nine being foreign), civic (10, several of these "Tax Payers"), social (5), and miscellaneous (8). Three societies awarded him honorary membership and one an honorary presidency; he was a Gold Medalist (1923) of the National Institute of Social Sciences; he was elected to the National Academy of Sciences in 1912 and to the American Philosophical Society in 1907.

The boy editor never stopped editing. He was on the editorial boards of Biometrika, Journal of Experimental Zoology, Zeitschrift Für Rassenkunde, Zeitschrift Für Menschliche Vererbung Und Konstitutionslehre, Psyche, Journal of Physical Anthropology, Eugenical News (which he started in 1916), Growth, and Human Biology. His projected Archives of Heredity and Variation, and Biological Journal did not appear.

The real estate office boy, as noted by MacDowell, never lost his delight in real estate transactions. He knew procedures; he could write a deed and survey a lot. At Harvard he bought for his family a house and lot that kept him in debt for many years. A lot was bought in Chicago early in 1902; six acres and a house on the shore at Cold Spring Harbor were bought personally in March, 1904, to rent to laboratory personnel and to build apartment houses for them. Next year a 19-acre farm was purchased in Mrs. Davenport's name, and property was later bought in nearby Syosset. The last purchase that Davenport negotiated was a tract of 32 acres for the Biological Laboratory. He so-

licited funds, arranged an exchange of Carnegie Institution property, steered the transaction, and had the property mapped out for a new community of biologists' homes. During the next twelve years only one such home was actually built.

On retirement (1934) a room in the Eugenics Record Office was assigned to Davenport, who became an Associate of the Carnegie Institution, with a grant for the completion of his anthropometrical studies. During his last ten years he wrote forty-seven papers, a book, and the fourth edition of his "Statistical Methods." Three of these papers were published posthumously with the editorial supervision of Dr. Morris Steggerda. He took an active part in civilian defense as an Air Raid Warden of the Nassau County Defense Council, and gave many hours to plane spotting as a Recognized Observer of the Ground Observers Corps of the Army Air Forces. Davenport was survived by his wife, and by his two daughters, Mrs. Millia Crotty Harkavy and Mrs. Jane Joralemon di Tomasi.

During his last years a district school was built across the road from his home. Here he spent much time talking to the classes on many subjects. He would take the children to gather frogs' eggs each spring; they would eat their lunch together by the pond and have a happy time. With them there was no defense and no embarrassment, and here he was content. Those children probably saw more clearly than anyone else the relaxed, unrepressed Charles Davenport, and they adored him.

In January 1944, it was learned that a killer whale had been beached at the eastern end of Long Island, and Davenport determined to secure its skull for the Whaling Museum at Cold Spring Harbor, of which he was Curator and Director. Many difficulties were overcome in moving the head to his home. Instead of using the slow but easy method of maceration in a pond he undertook to boil it, and for a fortnight he labored far into the nights in the intense heat of a cauldron in an open shed, with the bitter winter pressing in from all sides. He caught a cold and still worked on. The job was far from finished, when pneumonia developed. He had asked too much of his great ability to work. He died on February 18, 1944.

KEY TO ABBREVIATIONS IN BIBLIOGRAPHY

Amer. Anthrop. = American Anthropologist
Amer. Breeders' Mag. = American Breeders' Magazine
Amer. Jour. Insanity = American Journal of Insanity
Amer. Jour. Med. Sci. = American Journal of Medical Sciences
Amer. Jour. Men. Def. = American Journal of Mental Deficiency
Amer. Jour. Phys. Anthrop. = American Journal of Physical Anthropology
Amer. Jour. Roent. Rad. Therapy = American Journal of Roentgen Radiation Therapy
Amer. Nat. = American Naturalist
Amer. Statis. Assoc. = American Statistical Association
Anat. Anz. = Anatomischer Anzeiger
Ann. Amer. Acad. Pol. Soc. Sci. = Annals, American Academy of Political and Social Science
Ann. Rev. Physiol. = Annual Review of Physiology
Anthrop. Anz. = Anthropologischer Anzeiger
Arch. f. Ent. Mech. = Archiv für Entwickelungsmechanik
Arch. Internal Med. = Archives of Internal Medicine
Arch. Neurol. Psych. = Archives of Neurology and Psychiatry
Arch. f. Rass.-und Gesellschaftsbiologie = Archiv für Rassen und Gesellschaftsbiologie
Arch. Surg. = Archives of Surgery
Assoc. Res. Nerv. Mental Dis. = Association for Research in Nervous and Mental Diseases
Biog. Mem. Nat. Acad. Sci. = Biographical Memoirs, National Academy of Sciences
Biol. Bull. = Biological Bulletin
Bull. Amer. Acad. Med. = Bulletin, American Academy of Medicine
Bull. Brooklyn Inst. Arts Sci. = Bulletin, Brooklyn Institute of Arts and Sciences
Bull. Mus. Comp. Zool. = Bulletin, Museum of Comparative Zoology
Carnegie Inst. Wash. Pub. = Carnegie Institution of Washington, Publication
Carnegie Inst. Wash. Year Book = Carnegie Institution of Washington Year Book
Contrib. Embryol. = Contributions to Embryology
Educ. Rev. = Educational Review
Eugenics Res. Assoc. = Eugenics Research Association
Harvard Grad. Mag. = Harvard Graduate Magazine
Human Biol. = Human Biology
Internat. Rev. d. ges. Hydrobiol. u. Hydrogr. = International Revue des gesampte Hydrobiologie und Hydrographie

Jahrb. f. wissen. und praktische Tierzucht = Jahrbuch für wissenschaftliche und praktische Tierzucht
Jour. Agric. Res. = Journal of Agricultural Research
Jour. Amer. Med. Assoc. = Journal of the American Medical Association
Jour. Applied Psychol.= Journal of Applied Psychology
Jour. Dental Res. = Journal of Dental Research
Jour. Exper. Zool. = Journal of Experimental Zoology
Jour. Gen. Physiol. = Journal of General Physiology
Jour. Hered. = Journal of Heredity
Jour. Mam. = Journal of Mammalogy
Jour. Morph. = Journal of Morphology
Jour. Nat. Inst. Soc. Sci. = Journal of the National Institute of Social Sciences
Jour. Nerv. Mental Dis. = Journal of Nervous and Mental Disease
Jour. Physiol. = Journal of Physiology
Jour. Wash. Acad. Sci. = Journal of the Washington Academy of Sciences
Med. Rec. = Medical Record
Mem. Nat. Acad. Sci. = Memoirs, National Academy of Sciences
Nat. Educ. Assoc. U. S. = National Educational Association of the United States
Nat. Hist. = Natural History
N. H. Agric. Exper. Sta. Tech. Bull. = New Hampshire Agricultural Experiment Station, Technical Bulletin
N. Y. Med. Jour. = New York Medical Journal
Pop. Sci. Mo. = Popular Science Monthly
Proc. Amer. Acad. Arts Sci. = Proceedings of the American Academy of Arts and Sciences
Proc. Amer. Breeders' Assoc. = Proceedings of the American Breeders' Association
Proc. Amer. Phil. Soc. = Proceedings of the American Philosophical Society
Proc. Assoc. Res. Nerv. Mental Dis. = Proceedings of the Association for research in Nervous and Mental Diseases
Proc. Boston Soc. Nat. Hist. = Proceedings of the Boston Society of Natural History
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Proc. Soc. Exper. Biol. Med. = Proceedings of the Society for Experimental Biology and Medicine
Proc. U. S. Nat. Mus. = Proceedings of the United States National Museum
Proc. Wash. Acad. Sci. = Proceedings of the Washington Academy of Sciences
Psych. Bull. = Psychiatric Bulletin
Psychol. Bull. = Psychological Bulletin

Rep. Amer. Breeders' Assoc. = Reports of the American Breeders' Association
Roy. Hort. Soc. = Royal Horticultural Society
Sci. Amer. = Scientific American
Sci. Mo. = Scientific Monthly
Zeitschr. f. induk. Abstammungs u. Vererbungslehre = Zeitschrift fur induktiv Abstammungs und Vererbungslehre
Zeitschr. f. Morph. u. Anthrop. = Zeitschrift für Morphologie und Anthropologie
Zoöl. Anz. = Zoölogische Anzeiger

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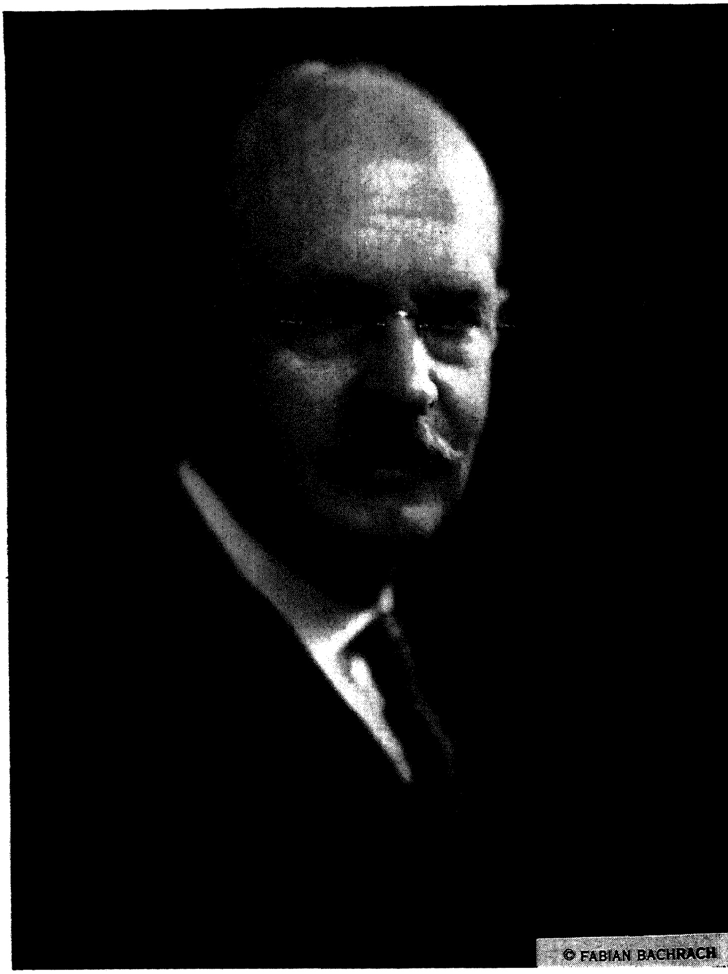

Frank M. Chapman

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Heredity, Environment and Culture

Frank Michler Chapman was born in what is now West Englewood, New Jersey, on June 12, 1864. The family home was an ample country residence, with wide lawns, fine old trees and high formal gate posts. This home was set in the midst of a large and prosperous farm, in a region abounding in woods and ponds, orchards and wide fields. The region was then a veritable paradise for birds; it was also an ideal environment for a child who grew up to be a most eloquent apostle of the birds and a faunal naturalist of immense achievement.

The home farm had been purchased from its Jersey Dutch owners in 1863. It was successfully run and developed under the general supervision of Frank's maternal grandfather, Chester Parkhurst. He was a retired physician whose ancestors came from Chelmsford, Essex, England.

Our best sources of biographic material are Chapman's delightful *Autobiography of a Bird Lover* (1933), his *Camps and Cruises of an Ornithologist* (1908) and his array of books, monographs and special reports upon the birds and bird faunas of North and South America.

Frank's mother, Mary Augusta (Parkhurst) Chapman was an ardent lover of flowers and a "born musician." "Always," he writes, "she had a garden and at times a small conservatory, and music was as much a part of our daily life as food." Her mother, Mary (Johnson) Parkhurst was also a "born musician." "It appears" (*Autobiography*) "that I have to thank . . . my mother and her mother for that love of music through which birds make their strongest appeal to me. Long before I had any desire to know their names I was deeply responsive to the songs of birds. Doubtless this pronounced trait, combined with a love of hunting which is the birthright of most boys, may

have helped to produce that intense interest in birds which is my most distinguishing characteristic."

Frank's father, Lebbeus Chapman, Jr., was the senior member of a New York law firm and counsel for a large bank. He was a veteran of the Civil War, a very busy and much sought-for man, with many professional and civic duties. But until the time of his death in 1876, he was the close friend of his young son. His father, in turn, was a banker and was the author of *Chapman's Interest Tables.* The incredible industry and application of this man is shown by a neat manuscript copy of the entire Bible, comprising 1467 quarto pages.

Such in general were the parents, relatives and ancestors of which Chapman later wrote—English on both sides, so far as he could learn, for three centuries past, except for one lone Irishman. The latter, we may imagine, contributed something to Chapman's sensitivity and to his love for lyric music. Chapman's middle name, Michler, a friend informs me, is that of an old Pennsylvania family of German origin which migrated southward into Georgia. Nathaniel Michler who wrote the report on a survey for a Panama canal (1861) was a close friend of Chapman's father.

As a small boy Frank read the *Leatherstocking Tales* and similar stories. He loved to roam in the woods, listening to every bird call to which his inner ears were keenly attuned, and doubtless stealthily following and approaching the elusive birds. When cherries were ripe he used to perch quietly in a certain cherry tree and, as he says, "share the feast with the Cedar Wax Wings." Or he would climb the rafters of the barn to the cupola and peer out for new arrivals.

But by what steps did this childhood play lead so directly to his career as an outstanding ornithologist, as a leader in science, in education, and even in social service? His deep love of music, his strong emotional response to the thrill of a bird-song and to the lure of a bird's flashing image were leading factors in his development; but so also were the strength and persistence of his hunting and collecting tendencies, together with his progressively constructive intelligence, which delighted in

planning and making or organizing something new, from a kite or a chicken coop to a boy's baseball club. From his birth onwards he was surrounded by a loving and appreciative family, parents, aunts, uncles and a beloved sister, who collectively protected him from the distracting man-made life in the city and consistently encouraged his interest in birds. Nor did they fail to encourage in him friendliness with modesty, good manners and a strong sense of fairness.

For ten years he attended school at Englewood (except for one term in Baltimore). At the Englewood Academy he was taught what were then considered to be the essentials of a sound education. When he graduated, at the age of sixteen, he did not choose to go to college and evinced a lack of enthusiasm for a system of education in which Nature, as a system, was practically ignored. At that youthful age, already thirsting for more and wider contacts with the world of birds, he perhaps did not realize that especially during the years at the Englewood Academy he was learning at least to write English well and clearly, or that he was acquiring the conventional background upon which he built so well his later career as author, lecturer and scientist. Nevertheless, it was no doubt fortunate that he did not go to college, for if he had done so he would have been more completely conditioned by the routine classical education of the period of 1880. His early drive toward bird study, bird hunting and exploration might have been crowded out by other interests. Moreover a college education might have interfered with his own highly productive results in self-education, in which his studies were motivated by his expanding needs.

As a young and isolated student of birds he had to learn almost everything by experience, gradually picking up the local names of birds, as well as the technical terms of ornithology. A kind uncle gave him Johnson's *Natural History*, in two large volumes with 1500 woodcuts and for ten years this was his only bird book. But its well studied text prepared him to appreciate the *Key to North American Birds* by Elliott Coues, which he acquired later.

BUSINESS VERSUS VOCATION (1880-1886)

In 1880, four years after the death of his father, he graduated from the Englewood Academy at the age of sixteen. "I had no call," he relates, "for any work or profession, my school life had awakened no special interests, but it was essential that I do something and, in default of other openings, I entered the American Exchange National Bank of New York, of which my father had been counsel. I remained in this bank for six years as a member of its city collection department. For the first time I now learned the meaning of the word work. In order to reach my desk at 9 a.m. I was obliged to leave home at 7:30 and the day's duties were not finished until our current accounts were proved. Usually this was about 5 o'clock, but it was often later and there were periods in 'coupon time' when night after night I did not leave the bank before midnight.

"The work was diversified, not without interest, and brought a growing measure of responsibility. It gave me a general knowledge of business methods, taught me to be prompt and made me far more orderly than I had been before—all good training for a naturalist and particularly for a museum man.

"Saturday half-holidays were then unknown and, aside from my annual vacation of ten days, Sundays and holidays were my only free days. There was, therefore, small opportunity for the development of my interest in birds. But on my journeys to and from New York I frequently met on the train Frederick J. Dixon of Hackensack, a man about ten years older than myself who knew more about birds than anyone with whom I had previously come in contact. He had a copy of the first (1872) edition of Coues' Key and a small collection of exceedingly well mounted birds. Furthermore he was a genuine lover of nature and a man of much personal charm. In him I found for the first time someone who fully understood my tastes and with whom I could talk birds. He could answer questions; he taught me how to make a bird skin."

A hardly less important link to his own future was supplied by his friend Clarence B. Riker, who could mount birds well

and who later made two journeys up the Amazon River to collect birds. Gradually the bonds that tied him to ornithology strengthened, even as his effective labors in the bank business brought him advancement there.

In the spring months of 1884, while still working full time at the bank he took part, as a volunteer, in a survey of bird migrations, initiated in Washington. The Atlantic Division of the Committee on Bird Migration of the American Ornithologists' Union was in charge of Dr. A. K. Fisher to whom Chapman ventured to offer his services. "This step," he writes (op. cit. p. 32), "determined my future. For the first time I was brought into direct communication with a professional ornithologist, and I doubt if one could have been found who would have shown more patience with my ignorance or devoted more time to my guidance. In a close friendship, which has now extended nearly fifty years, I have always called Dr. Fisher my 'ornithological godfather'."

But how was he to secure the necessary data on the migratory movements of birds and put in a full day's work at the bank? "Fortunately," he writes, "my environment permitted me to serve both birds and Mammon. At this time the station of West Englewood, on the West Shore Railroad, distant about half a mile from my home, was in the heart of as good collecting ground as there was in the New York City region. The woods surrounding it stretched for miles north and south, forming a highway for the diurnal journeys of migrating birds. The numerous roads and farm lanes of the Phelps' estate made them as easily penetrable as a park. A novice could not have found a better place in which to record the migration, supplementing his observations, when need arose, with specimens.

"From March 10th to May 23rd following, with the exception of Sundays and one day off, I went through the motions of a bank clerk, but for the whole period I lived, thought and dreamed the life of a bird student. Each morning I arose at daybreak. A cup of coffee, made the night before and heated over an alcohol lamp while I dressed, helped me to swallow two slices of bread. Then with my gun I was off for the

woods. My route was planned to bring me to the railroad station at 7:30 when the dress of the hunter was hurriedly changed for that of the bank clerk and I boarded the 7:39 train to begin what seemed like another existence.

"At night, on returning from the city, if time permitted, I again went to the woods for a brief outing before dinner. After dinner there were specimens to skin and notes to write, when, without urging, I went to bed as part of the preparation for the next day.

"My records, kept by the roll call and journal system, show that of the seventy-five days included in the period mentioned I went afield sixty-nine, for a total of 171 hours and 21 minutes' observations, or an average of two hours and twenty-nine minutes daily. Besides Sundays I was absent from the bank only one day (May 15th), which I celebrated by collecting a specimen of Brewster's Warbler, the climax of an experience which, in absorbing interest and stimulating excitement, has not been equaled in the succeeding nearly fifty years. . . ."

His report on 103 species from the restricted West Shore area was finished on time and forwarded to Dr. Fisher. "In due time came his official acknowledgment. The verdict of my initial venture in ornithology was in my hands. Had I succeeded or failed? I hesitated to learn, and it was not until I had reached the seclusion of the orchard that I ventured to open it. I recall the apple tree under which I stood when with inexpressible elation I read that my report was the best one that had been received from the Atlantic District."

Freedom at Last (1886)

For two years longer he continued to serve the bank, no doubt faithfully and well, but on his Sundays, holidays and evenings he was building up his contacts with the worlds of birds and men. He joined actively in the campaign of the National Audubon Society against the devastating attack upon bird life of the millinery trade, which was engaged in wholesale slaughter of nesting egrets, thrushes, warblers and other birds. Especially after his promotion to be in charge of the

bank's city collecting department it became evident, he notes "that there would soon be a serious conflict between the bank clerk and the bird man." In the fall of 1886, with the full consent and cooperation of his mother and in no way disturbed by the "forebodings" of his "mystified colleagues," he resigned his position in the bank.

At last he was free to attend the Congress of the Ornithologists' Union in Washington, to lay plans for collecting in Florida, to fit up his workroom in his mother's winter home at Gainesville amid the pines and sand of North Central Florida. At Alachua Lake, near by, he was thrilled by the diversity of land and water birds. By May 1887 his collection included 581 Florida birds; and he had trapped many gophers on what is now the site of the University of Florida.

Returning to Englewood for the summer months he took his collection to the American Museum of Natural History for study and identification, sharing a room there by invitation of Mr. George B. Sennett, of Erie, Pennsylvania. Mr. Sennett had a considerable collection of Texas birds, upon which he was preparing a book. After Chapman had completed the assortment of his own collection, he devoted one half of each work day to assisting Mr. Sennett for the sum of one dollar a day. The other half of each day he gave to Dr. J. A. Allen, the curator of birds and mammals, who was then happily immersed in assorting the Lawrence Collection of 8,000 Central and South American birds.

"The days were now too short. Although master of my own time I traveled on the same trains to and from Englewood that I had used as a bank clerk. But with what a difference I approached my desk . . . No two days were alike, the character of the work might be the same but the birds were different. Actually dead specimens, they carried the message of living ones . . ." In other words he read these "feathered documents," as he afterward called them, with as clear a purpose as that of an epigrapher deciphering an inscription or of a palaeontologist studying a trilobite.

His first scientific paper, published in the Auk in January, 1888, recorded the results of his observations on the nocturnal migration of birds, as seen through a friend's 6-inch telescope against the glowing surface of the full moon's disk. The paper was presented at a meeting of the American Ornithologists' Union and was the first of about two hundred technical papers and notes, in addition to twenty books. About this time he was already taking photographs of living birds, using "a crude shutter made from pieces of a cigarbox and a rubber band," and thereafter many of his books and papers included his beautiful photographs of birds in their own environment.

Museum Call (1888)

He was about to start, with Charles B. Cory, in a sternwheel flat-bottom houseboat, on a collecting expedition to Lake Okeechobee, Florida, when he received a climactic letter. "It was from Dr. J. A. Allen offering the position of his assistant in the American Museum. I read it at the lunch table and it was several moments before I could sufficiently compose myself to tell my companions [the Corys] of its contents." Without hesitating he resigned from the Okeechobee party and, taking the first available train north, reported for duty at the Museum March 1, 1888.

At the Museum he continued the cataloguing and identification of bird collections upon which he had previously worked as a volunteer. "Then began an association [with Dr. Allen], which so far as our professional relations were concerned, was like that of father and son, rather than that of a curator and his assistant." Chapman modestly says: "I came to Dr. Allen with only a beginner's knowledge of local birds and had everything to learn concerning the more technical side of ornithology. There was no one under whom I could have worked and studied more profitably. For nearly twenty years our desks were side by side." Dr. Allen in turn found in Chapman the ideal assistant and colleague.

Then followed more than half a century of crowded joyous years, with ever widening activities and higher horizons both

for the "apostle of the birds" and for those who came under his genial influence.

At the time when Dr. Allen called Chapman to be his assistant the American Museum department of birds and mammals had a large exhibition hall containing many thousands of birds, each one mounted on its own stand, arranged systematically in seemingly endless rows, on bluish white shelves. Experience gradually showed that this "dictionary type" of massing soon tired almost everyone except the bird expert. Before long Dr. Allen delegated to his willing assistant the problem of improving the educational value of the Museum's bird exhibits.

Chapman early began to install an exhibit of the birds of New York City and environs. Later he divided the great mass of birds in the central bird hall into two sections on opposite sides of the hall. On one side, the birds were arranged according to major geographic regions, on the opposite, according to their supposed relationship in orders, families, genera and species.

After a while he began to deliver popular lectures on birds, illustrated largely with lantern slides from his own photographs. These slides, he tells us, had been beautifully colored by his mother who continued to aid and encourage him in all his labors. In the winter seasons, starting from his mother's home in Gainesville, Florida, he made numerous field trips to collect small mammals and birds; thus he went to various localities in Florida, Texas, Cuba, Trinidad, B. W. I., Yucatan and Vera Cruz, Mexico, and later to many countries in South America. The story of his local expeditions in the United States and of his one visit to England is told in his *Camps and Cruises of an Ornithologist* (1908) and much later his many expeditions to Mexico, Central and South America are dealt with in his all too brief, authentic *Autobiography of a Bird Lover* (1933). Unfortunately, in the present short sketch of his amazingly diversified and yet unified career these earlier expeditions can only be referred to in passing.

The American Ornithologists' Union and the Audubon Society

Chapman took a very active part in both these organizations soon after they were founded; the former in 1883. In 1894 he was appointed associate editor of *The Auk,* organ of the A.O.U., in 1899 he established *Bird-Lore* for the Audubon Society, serving continuously as its editor for more than 34 years. He also served actively on the Board of Directors of the National Audubon Society, and was elected its Honorary President in 1945.

Mrs. Chapman "Signs On" (1898)

At the beginning of the second decade of his Museum life Chapman was married (February, 1898) to Fannie Bates Embury. Their wedding journey was to Oak Lodge on the Indian River, Florida, conveniently near a certain island which yielded a series of a rare species of sparrow. At Mrs. Chapman's suggestion materials for a Brown Pelican group were also collected near by. To Chapman's "mixed astonishment and joy" his bride proved to be singularly adept in skinning both small birds and large; she was, in fact, an ideal assistant preparator and, like his mother, she devoted herself to furthering his career. A year later (1899) the Chapmans journeyed north to Bird Rocks in the Gulf of Saint Lawrence, where they collected materials and photographs for the group illustrating colonial bird life of the auks, gannets and other oceanic birds.

During his first ten years at the Museum (1888-1897) Chapman published thirty-four papers on birds and eleven on mammals. His *Handbook of Birds of Eastern North America* (1895) and his *Bird-Life: A Guide to the Study of Our Common Birds* (1897) also fall within this decade, as does his *Visitor's Guide to the Collection of Birds Found within Fifty Miles of New York City.* The *Handbook of Birds of Eastern North America* (1895, 1932) won high praise from Elliott Coues, William Brewster and other authorities and went through several editions. All his popular books and papers were welcomed

alike by scientists and amateurs and they opened doors to the world of birds for thousands of intelligent readers. Several of his papers dealt with bird migration and its origin, which was one of the main problems that led to his far-flung expeditions.

The Gallery of North American Birds

Early in the "second period" (1898-1910) of his Museum service, by enlisting the generous support of John L. Cadwalader, he was able to make a marked advance in the art of bringing into the Museum the illusion of outdoors, at the same time showing the bird in relation to its haunts. This ambitious plan began to be realized in 1901 in the relatively large mounted group illustrating the seashore of Cobb's Island, Virginia, with its nesting skimmers and other birds. The adult Black Skimmers are conspicuous against the sand, but their protectively colored young, when spreading out flat at the danger call of their mother, are very hard to detect, even at close range. The painted background in this habitat group was a novelty and was criticized by some as too dramatic. But no one could say it was not true to Nature, and under Chapman's hands the painted background for large groups was steadily developed.

The success of the Cobb's Island group brought added support for Chapman's project for an entire gallery of habitat mounted groups of North American birds in their natural environment. For the completion of this project ten years of work and many field trips were necessary. The visitors to this gallery make, as it were, an extended tour of North America from the Palisades of the Hudson River to the Bird Rocks of the Saint Lawrence River, through the southern and western states to California and Mexico. The most imposing group is that showing the flamingos of the Bahamas in the nesting season. At the time these groups were constructed they were inevitably placed in this gallery, which has low ceilings. The potentially majestic scene of Mt. Orizaba, Mexico, was thus severely cramped. But it served to point the need for much greater dimensions for this and similar types of Museum exhibits.

After more than forty years all the groups of the North American bird gallery are still authentic and invaluable records both of the birds themselves and of the regions in which they lived. Most unfortunately the main entrance to the gallery in which these exhibits are installed is located in a part of the Museum which is now reached by few visitors, and of these fewer still pass through the dark gallery.

The Warblers of North America (1907)

After accumulating data on the warblers for many years Chapman, with characteristic fairness, had printed on the title page "By Frank M. Chapman with the cooperation of other ornithologists."

All 55 of the North American species were accurately and briefly defined, including those whose nearest relatives are found in South America. The males and females of the North American species were illustrated by clear-cut portraits in color by Louis Agassiz Fuertes.

"The wide range of some species," Chapman writes (ibid., page 11) "makes a geographical analysis of the group difficult, but by alloting a species to the region in which it occupies the largest area we have the following results:

South America	40	species
Galapagos	10	"
Central America and Mexico	30	"
West Indies	20	"
North America	55	"

Part of the introductory chapter which dealt with the migrations of warblers was written by W. W. Cooke. He notes that the Blackpolls, which are the greatest travelers among warblers, make annual migration trips of between 3500 and 7000 miles to their winter area in Brazil and Chile; also that thousands of warblers perish in storms by attempting to cross the Gulf of Mexico in a single flight. But why do these persistent little birds continue to follow this dangerous route instead of skirting the shores along a presumably safer way?

It was largely to secure answers to this and hundreds of more detailed questions that Chapman journeyed to so many parts of North and South America and sent his coworkers and assistants to many others.

Chapman and Fuertes

Among the numerous artists who went with Chapman into the field and painted backgrounds and bird portraits in the Museum none was ever so close to him as Louis Agassiz Fuertes of Ithaca, New York. For Fuertes, Chapman had an unbounded admiration. Chapman himself was no mean bird mimic, but he was amazed at Fuertes' wizardry in calling new birds; he rejoiced in sharing with the latter the adventures of the Andean mountain trails; he appreciated the tact and diplomacy shown by Fuertes in resounding Spanish phrases when dealing with pompous customs officials. Finally, Chapman was proud of his younger friend's remarkable deftness and productivity in turning out bird portraits of impeccable accuracy and with all the sprightly outlook of the bird in life. When many years later Fuertes was killed (1927) in an automobile crash, Chapman's sorrow was most grievous.

Faunal Zones of the Andes

In two of his earlier journeys in Mexico Chapman was deeply impressed by the bird life of Mt. Orizaba which was stratified in successive zones, each with its characteristic bird fauna. Humboldt had observed the same phenomenon in the Andes but Stolzmann in Peru was practically the only one who had made accurate observations in this field (Chapman 1933, p. 208). Humboldt indeed "in determining the relations between altitudinal and latitudinal climates had shown that as we proceed from the Equator toward the poles the mean temperature decreases one degree Fahrenheit with each degree of latitude. But as we ascend a mountain, the mean temperature decreases one degree with each 300 feet of altitude. That is approximately 300,000 feet of latitude equal 300 feet of altitude. Climatically therefore, we travel about one thousand times faster vertically

than we do horizontally." Now the Andes, Chapman reasoned, "did not attain their full elevation until the latter part of the Tertiary," but [from indirect evidence] it is probable that the bird-life of South America, "at least in its major aspects did not [then] differ materially from that which exists there today. Hence it follows that . . . we can not only give the Andes a geological birthday but can form a fairly definite conception of the character of the avifauna from which the hundreds of species of birds that have evolved on them were derived." "It is consequently obvious," Chapman continues, "that in a study of the origin of life in the Andes we can associate cause and effect far more frequently than in those continental areas the early pages of whose geological and biological history are lost in an incalculably remote past. One asks, therefore, what are the factors that determine with such clearness the boundaries of these Andean life zones? Whence came the hundreds of species that are confined to them?"

Such were the general principles and basic problem when in 1911 Chapman's explorations in the Andes began. During six years of exploration which were largely supported by friends, especially Malcolm MacKay, Graham Sumner, George B. Case and Daniel E. Pomeroy, the Andes were crossed and recrossed many times and in various countries, especially Colombia and Ecuador. Many thousands of birds were collected: (1) in the humid Tropical Zone, extending to elevations of from 3,500 to 5,000 feet, (2) in the Sub-Tropical Zone, up to 8,000 to 9,500 feet, (3) in the humid or arid Temperate Zone reaching up to 11,000 to 12,000 feet and (4) in the relatively barren Páramo or Puna Zone, which extends to the lower level of snow, or, usually about 15,000 feet. Both the flora and fauna of the Páramo Zone are remarkably distinct. Nearly all its birds are endemic.

The gradual recognition of these bird zones in Colombia and Ecuador came as the result of an enormous amount of field work and Museum study, as fully set forth in Chapman's monographs on the distribution of bird-life in Colombia (1917) and in Ecuador (1926). These zones, to Chapman's great satis-

faction (Autobiography, p. 211) agreed essentially with those of Wolf in the latter's work on the distribution of plants in Ecuador. After prolonged studies, Chapman concluded that generally speaking bird populations of the Sub-tropical and Humid Temperate zones had been derived respectively from their next lower zone. The Arid Temperate and Páramo birds, on the other hand, have been derived from birds of the *same* zones in adjacent or distant areas. If from far southward, below the Tropic of Capricorn, these zones will be at a lower elevation, perhaps reaching sea-level at the southernmost point; but they are not "lower zones," strictly speaking. "The apparent ancestor of a subtropical, equatorial Motmot, for example, was found in the tropical zone of eastern Mexico. Again, an Ovenbird (*Cinclodes*) of the Colombian Páramo has evidently originated in Patagonia."

In 1918 the National Academy of Sciences awarded Chapman the Daniel Giraud Elliot Medal in recognition of his first attempt to present the problems of zonal evolution. Three years later, presumably in further acknowledgment of his work on the Andes, he was elected to membership in the Academy. Other medals and honorary membership in learned societies came to him in due course.

The Red Cross

Chapman's ability to tell a great and true story in a modest, factual but thrilling way is seen in his brief account of two years spent by himself and Mrs. Chapman in the service of the Red Cross (Autobiography, pp. 274-300). He pays the highest tribute to the leadership and organizing ability of Henry P. Davison. Mr. Davison was appointed by President Wilson to be Wartime Director of the Red Cross. In a relatively short time he created out of near-chaos an enormous but effective organization of 22 million members; under his leadership over 200 million dollars were raised for the relief of suffering in the afflicted countries of Europe.

Chapman's job was Director of the Department of Publication, for which much of his training as editor and writer and

as director of large enterprises well qualified him. Mrs. Chapman had charge of women's work in the Potomac Division of the Red Cross.

On account of Chapman's wide knowledge of Latin American countries and their peoples, he was dispatched to the West Indies and South America as special commissioner. For this purpose he wrote many pamphlets, designed posters and prepared lectures. "While my Spanish heretofore had served well as a means of communication with guides, cooks and mule drivers," he writes (ibid., p. 279), "I was not prepared to enter the diplomatic and oratorical field." He and Mrs. Chapman therefore added to their daily duties at the Washington headquarters a nightly course in Spanish. Equipped also with lantern slides and motion pictures, they journeyed to Cuba, Panama, Peru, Chile, Bolivia, Argentina, Uruguay, Brazil. Everywhere the responses to their Red Cross appeal by the people in these countries were highly satisfactory and productive.

Their next journey was to France. At Gièvres, near Tours, they met their son Frank M., Jr., who had enlisted in the marines and had been in the hospital in Gièvres.

Joining Mr. Davison in Paris, they found him organizing a league of Red Cross societies and even beginning to plan for an international organization for the maintenance of peace.

After the armistice they went to England, where they were impressed by the love of outdoor life and of native birds shown by a war-weary nation. Chapman and his friend Frederic C. Walcott then took the opportunity to renew their acquaintance with the common British birds.

MTS. RORAIMA, DUIDA

Chapman's paper on "The Upper Zonal Bird-life of Mts. Roraima and Duida" (1931) is essentially a comparative analysis of considerable collections of South American birds from two stations: Mt. Roraima at the junction of British Guiana, Brazil and Venezuela, and Mt. Duida in Venezuela, 400 miles south-southwest of Mt. Roraima. The collections

from Mt. Roraima were made by the Lee Garnett Day Expedition, under G. H. H. Tate, while those from Mt. Duida were made by the Tyler Expedition, also under G. H. H. Tate. About 161 species or subspecies representing 35 families were studied.

According to Tate's field observations and papers, both Roraima and Duida are table mountains, steep-sided and formed of sandstone, but the strata of Roraima are level bedded, while those of Duida are intensely folded. Roraima and its neighbors are considered remnants of a once continuous tableland which became dissected by erosion and is now represented by isolated fragments. Duida appears to be a newly up-faulted mass perhaps still rising. Roraima is almost devoid of soil and is the home of a relatively small and apparently diminishing plant and animal population. Duida has a deep covering of humus and bears a complex and highly modified fauna and flora. In spite of their separation and of the above noted differences, Chapman's analyses led him to the conclusions (1) that the avifauna of the upper zone of Roraima was essentially like that of Duida; (2) this distinctive upper zonal Roraima-Duidan avifauna contained the following chief components:

Of tropical origin..................	22 out of 85
With Andean relationships..........	39 " " "
With Southeastern Brazilian relationships.....................	2 " " "
With Guatemalan relationships......	2 " " "
Of unknown origin................	20 " " "

Thus nearly one-half the birds peculiar to the Roraima-Duidan fauna have their nearest relatives in the Subtropical and Temperate Zones of the Andes (p. 42). "These birds find their nearest relatives at a distance of usually more than a thousand miles, nevertheless they outnumbered those members of the Roraima-Duidan fauna derived from nearby tropical ancestors by more than 50 per cent. This fact indicates the greater age of the Andean element and also that when it was

acquired the Roraima-Duidan and Andean forms, or their antecedents, were more nearly in contact than they are today" (p. 45).

Further analysis indicates (p. 47) that at no very remote period the present wide gap in distribution between the Roraima-Duidan and Andean forms was bridged by intermediate stations which have since disappeared. The existence of Roraima-Duidan forms with close relatives in the Andean Subtropical and, less frequently, Temperate Zones "may be explained by the disappearance of their common ancestors or connecting forms in the intervening area, due probably to the influence of climatic changes" (p. 58).

Eastern Brazil

Extensive and much needed bird collections from eastern Brazil were made in a seven-year campaign by Emil Kaempfer, under the patronage of Mrs. E. M. B. Naumburg, Research Associate of the Museum's Department of Birds.

Marine Birds of Peru

Dr. Leonard C. Sanford and his collectors, especially Rollo Beck, had already made great collections of the marine birds that swarm in vast numbers along the coast of Peru, and Dr. R. C. Murphy studied the complex ecologic factors which made these dense bird populations possible.

"It is well known," writes Chapman, "that in this region, the combination of exhaustless supplies of fish, fish-eating birds (transformers, they might be called), islands on which birds may nest in safety, and a rainless climate has resulted in the production of vast quantities of guano. It is said that a billion dollars' worth of this fertilizer has been removed from Peru's coastal islands, and the annual deposit now constitutes one of her principal commercial assets. This subject, the biology of the Humboldt Current, and many other things of interest will be found fully treated in *The Bird Islands of Peru* by my colleague, Robert Cushman Murphy."

The Whitney Pacific Expedition

From 1920-1928 Rollo Beck carried on his explorations in the islands of the Pacific under a generous fund which, through Dr. Leonard C. Sanford, was contributed by Harry Payne Whitney. Beck amassed enormous collections and was succeeded by Hannibal Hamlin, who in turn (1930) was relieved by William F. Coultas.

All the South American marine birds in the Museum's collections were the sources for Dr. Murphy's two splendid volumes *Oceanic Birds of South America.*

African Birds

Meanwhile in Africa, Dr. James Chapin, De Witt Sage, J. Sterling Rockefeller and Charles B. G. Murphy made extensive collections, especially in the Belgian Congo. The vast bird fauna of the latter country was revised and described in two volumes by James Chapin, *Birds of the Belgian Congo;* a third and fourth volume are nearing completion.

The Lord Rothschild Collection and the Whitney Wing

Under Chapman's leadership the Museum's Department of Birds received in 1932 the supreme gift of the Lord Rothschild Collection of 280,000 specimens. With it came provision for its administration and for fellowships for students from abroad—all this was in memory of Harry Payne Whitney from his widow and children. Before Mr. Whitney's death in 1930 he had joined with the city of New York in giving an entire new wing to the Museum. This was completely equipped to house the vast study collections and to provide ample facilities for research. Provision was also made for the superb hall of oceanic birds and for another large hall containing a general introductory exhibit on the biology of birds.

In this palatial Whitney bird building Chapman chose an unpretentious office, adjacent to the long galleries containing part of the study collections. At convenient locations near other galleries were the offices of his well-trusted colleagues: Robert Cushman Murphy who succeeded him as Chairman of

the Department of Birds; John T. Zimmer, a widely trained ornithologist from the Field Museum; James Chapin, to whom the forests of the Belgian Congo were as a second home; Ernst Mayr, a former member of the Whitney South Sea Expedition and a distinguished student of bird speciation; Mrs. Elsie M. B. Naumburg, author of *The Birds of Matto Grosso* and other publications on South American ornithology, who had long assisted Chapman, especially in identifying the birds of Ecuador, Peru and Brazil.

Always in close communication with Chapman was his life-long friend Dr. Leonard C. Sanford, of New Haven, a trustee of the Museum and patron of expeditions. It was largely through his good offices that Harry Payne Whitney and later, the Whitney family, made their princely contributions to the Museum as above noted.

Barro Colorado Island

Whenever it was possible in his later years Chapman retired to Barro Colorado Island in the Panama Canal Zone. His abundant observations on the birds and mammals there are recorded in *My Tropical Air Castle* (1929) *Life in an Air Castle* (1938) and many later articles. This "island" resulted from the damming of the Chagres River. The latter flooded 165 square miles of lowland and made islands of hilltops. Mostly covered with forests it is the home of many birds, mammals and other animals and is under the protection of the U. S. Government. At first Chapman had only a small cabin on the island which, however, was ideally placed for a student of birds. Later Fuertes House was built and named for his deceased friend. From this high point of vantage Chapman could identify with his binoculars the water birds on Gatun Lake below him, or the soaring birds in the air above or the perching birds near by. Across the forest paths he set out photographic traps and when the bird or mammal touched the string the camera clicked. Thus he took beautiful portraits of ocelot, coatimundi, peccaries, tapir, et al. By tempting a savage male coatimundi

with bananas, he gradually succeeded in lessening its fear. Eventually "as an evidence of good faith on both sides," he held out the banana and the coati grabbed it, without attempting to use its dagger-like canine teeth on the donor. The life story of the male coati was eventually recorded and the role of the fighting male in coati survival was clearly shown.

Professional students of animal behavior rightly distrust the man-made interpretations which mar animal stories of casual observers. In his descriptions of the social life of the howler monkeys, Chapman used only common English words and metaphors, but an experimentalist who might wish to test the IQ of these nearly untamable savages, would do well to study Chapman's essentially objective records of their behavior both in the wild and in captivity. The strength of their dislike for strangers is suggested by the long stubborn and sullen resistance of a young female howler monkey against Chapman's persistent and finally successful attempts to gain her confidence.

While the reasons for the behavior of howler monkeys can to a certain extent be understood by us, the elaborate and fantastic courting antics of Gould's manakin, as recorded by Chapman, belong in the world of lizards and birds and are motivated and executed by a profoundly unmammalian nervous system. In brief, Chapman's books and articles on the fauna of Barro Colorado abound in material and problems which invite cooperative investigation on a large scale by geneticists, behaviorists and students of the evolution of the vertebrates.

Chapman Dies, His Work Survives

As Chapman neared the age of fourscore years his strength failed and sorrow increased, for Mrs. Chapman died in September, 1944. But his son, Major Frank M. Chapman, Jr., returning safely from World War II, brought him much happiness, as did his son's wife Gladys Swarthout Chapman. As they both were distinguished musicians, they gave him the solace of uplifting music. Nor did he forget the thrill of countless bird-songs stored in his memory.

Thus on November 15, 1945, he died, content to leave to his devoted successors and colleagues the great cause for which he had so long labored. And all the amazing things he saw and recorded in the world of birds are ready for generations of readers still to come.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Am. Mus. Jour. = American Museum of Natural History Journal
Am. Mus. Nov. = American Museum Novitates
Bull. Am. Mus. Nat. Hist. = Bulletin, American Museum of Natural History
Bull. U. S. Nat. Mus. = Bulletin, United States National Museum
Carnegie Inst. Wash. = Carnegie Institution of Washington
Century Mag. = Century Magazine
Educ. Rev. = Educational Review
Geogr. Jour. = Geographical Journal
Nat. Geogr. Mag. = National Geographic Magazine
Nat. Hist. = Natural History
Pop. Sci. Mo. = Popular Science Monthly
Proc. Biol. Soc. Wash. = Proceedings, Biological Society of Washington
Proc. Linnaean Soc. N. Y. = Proceedings, Linnaean Society of New York
Trans. N. Y. Acad. Sci. = Transactions, New York Academy of Sciences

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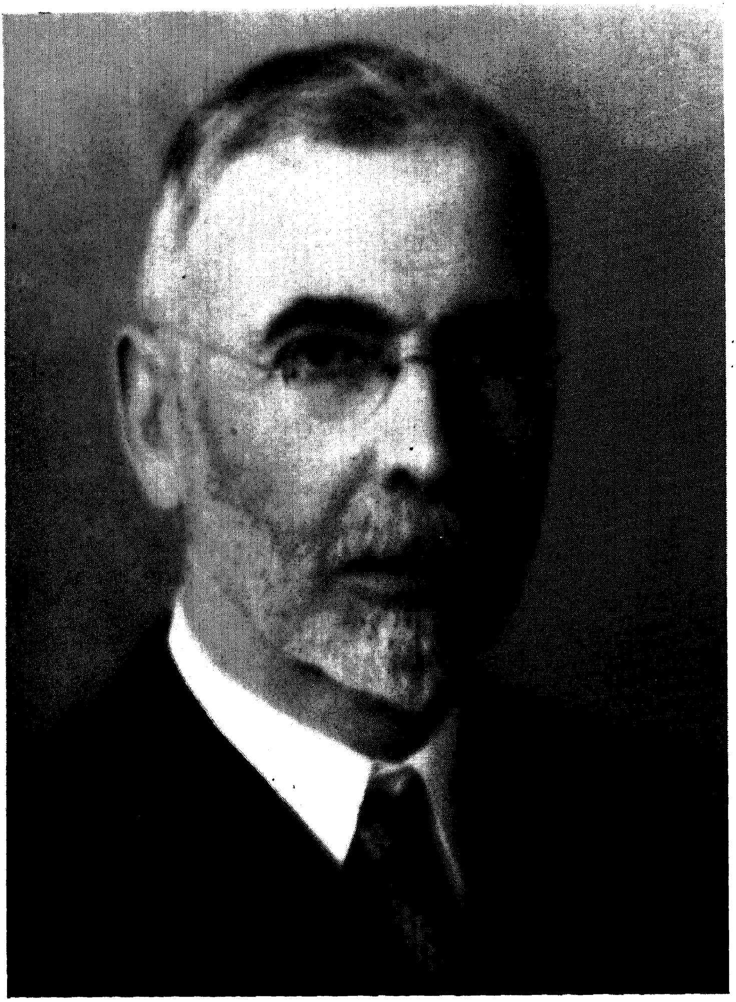

Francis B. Sumner

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OF

FRANCIS BERTODY SUMNER

1874–1945

BY

CHARLES MANNING CHILD

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1947

FRANCIS BERTODY SUMNER

1874–1945

BY CHARLES MANNING CHILD

No one who knew Francis Sumner could fail to be impressed by an outstanding characteristic of the man, his intellectual honesty, not only in regard to his particular fields of research, but in his wide general interest in human relations and problems, including those of his own personality. It is impossible to read his "Life History of an American Naturalist" without realizing that the self-analysis and self-criticism, perhaps the most interesting features of the book, are expressions of this characteristic. Not all men could have viewed themselves as objectively and have criticized themselves as frankly as he has done. If he has erred it is not in the direction of overestimation. This cannot be said of all autobiographies.[1]

Francis Bertody Sumner was born August 1, 1874, in Pomfret, Connecticut. His parents were Arthur and Mary Augusta (Upton) Sumner. His father's ancestry was predominantly English, but in the "Life History" he speaks of an interesting family tradition that an Italian, Bertoldi by name, an official at Ispahan, Persia (Iran), married a Persian. Their son served as a physician to the king of France, but later, during the period of the French Revolution emigrated to the United States and married a great-grandmother of Sumner in the paternal line. Sumner's face was regarded by E. W. Scripps, one of the founders of the Scripps Institution, as showing evidence of this Iranian ancestry. This seems to be something more than a mere family tradition. Mrs. Sumner possesses Francois Bertoldi's snuff box, also a copy of his appointment as physician to "Monsieur," later king of France, and the citation for an honorary degree at the University of Padua. Moreover, Dr. Sumner's middle name, "Bertody," is apparently derived from Bertoldi. His mother's ancestry was English. She had

[1] Acknowledgment is due to the Roland Press Co. of New York, present holders of the copyright, for permission to quote from "The Life History of an American Naturalist." The source of each quotation from the book is indicated.

taught in a school in Charleston, directed by his father. His father retired early from the teaching position and a few months after Sumner's birth the family went west and lived for about ten years on a somewhat isolated place at the foot of the hills back of Oakland, California. Here Sumner and his older brother ranged the hills and made collections of various sorts, kept insect larvae and learned about their metamorphoses and about the life histories of amphibians. Toads and snakes were Sumner's special pets. A real interest in living things, encouraged by his father, developed in this environment with the aid of a few elementary books on natural history and a small compound microscope.

During the Oakland period he did not attend school but was taught by his father. When he was ten years old the family began a reverse migration eastward, living in Colorado Springs for three years. There Sumner entered school, a private school, for the first time and found himself in advance of children his age, but not well adjusted as regarded social relations, since he was not accustomed to boys in large numbers. Here also he roamed the Garden of the Gods and nearby canyons and collected minerals and fossils, developing still further his interest in nature. Having been given a set of chemical reagents with directions for use he became interested during this period in "rather planless messing with chemicals" and decided to become a naturalist and chemist.

The next stop in the family migration eastward was Minneapolis where Sumner spent three years in schools and four (1890-1894) in the University of Minnesota. His chief interest was zoology and he regarded Dr. H. F. Nachtrieb as largely responsible for his final choice of zoology as a lifework. During two summers of this college period he was a member of field parties sent out from the university to collect biological material, particularly fishes, from the region about Minneapolis, which was then largely wild country with numerous lakes. These trips involved camp life, often much discomfort, particularly from mosquitoes, but during them he acquired extensive knowledge of field zoology and developed the interest in fishes which led later to some of his most interesting work.

A very considerable interest in psychology and philosophy was aroused during the college period and persisted throughout life. He has expressed himself as believing that with a broader outlook and more stimulation and enthusiasm on the part of his instructor, philosophy rather than zoology might have become his field of endeavor.

He was elected to Phi Beta Kappa, received the Bachelor of Science degree at the age of twenty, and was granted a graduate fellowship which, however, was not used because he was advised to delay further studies on account of his health.

Although Sumner has dealt in the "Life History" more at length than is possible here with his childhood, school and college periods this brief review of those parts of his life has seemed essential to an understanding of his later scientific career. His interest in nature from early childhood, encouraged, rather than ignored or discouraged by his father, became the foundation on which his lifework was built. To what extent that interest resulted from the relative isolation from social contact in his earlier life or from a certain type of reaction system he does not, and no one else can, attempt to decide. However, it appears that the environment was by no means unfavorable as a background for development of a biologist, though if he had reacted differently to it his later life might have been entirely changed.

After his graduation Sumner joined his family, then living in Westminster, Maryland, made various attempts to obtain some sort of position and saw Dr. Osler of Johns Hopkins, who suggested first rest, then a trip to South America on a sailing ship, which was accomplished with considerable discomfort and much of interest. On his return another physician advised his return to university work and in the autumn of 1895 he entered Columbia University as a graduate student with zoology as a major and physiology and psychology as minors. In zoology he came into contact with E. B. Wilson, Bashford Dean, and H. F. Osborn. During his first year at Columbia he was somewhat repelled by the amount of routine drudgery involved in various fields of zoological research, could not become interested in "cell lineage," considered at one time beginning a medical course, but with encouragement by Bashford Dean returned to the fishes

and undertook work in fish embryology. The entire development of a fish proved to be too extensive a field, for his thesis, entitled "Kupffer's Vesicle and its Relation to Gastrulation and Concrescence," was concerned only with certain features of development which, however, had a definite bearing on the general problem of formation of the fish embryo.

In psychology he came under the influence of Dr. Cattell, prepared a questionnaire on certain aspects of belief, treated the answers statistically and became particularly interested in Cattell's application of quantitative methods to the treatment of individual differences. This he regarded as influencing his use of essentially the same technique in his later work on animal variability. During this period he became impressed by reports on so-called psychical research, had an interview with William James and visited several "mediums."

Following an unsuccessful expedition by Reid Hunt and N. R. Harrington to the Egyptian Sudan in 1898 to obtain developmental stages of the lungfish, *Polypterus,* which were expected to throw light on the evolution of the land-living vertebrates, funds were provided for a second expedition to the Sudan in 1899. This consisted of Hunt, Harrington, and Sumner. After a brief stop at the Naples Zoological Station, Hunt and Sumner joined Harrington in Africa and the search for the desired material began but was unsuccessful. Hunt and Harrington developed fever almost at once and the expedition ended with Harrington's death and Sumner's illness. Another brief stay at the Naples Station in the autumn of 1899 after leaving Africa was ended by the notice of Sumner's appointment to the Natural History Department of the College of the City of New York. There he found conditions unbelievably primitive but held the position until 1906 with two-years' leave of absence. It was scientifically an unsatisfactory period, both because he felt himself ill adapted to teaching elementary biology with little evidence of interest on the part of students and because of the very limited possibilities for research. In consequence of this appointment he was unable to complete requirements for the degree of Doctor of Philosophy at Columbia until 1901. During the years as a graduate student at Columbia and as teacher at the College

of the City of New York most of the summers were spent in research at Woods Hole with one summer at the Naples Station and one at Cold Spring Harbor.

In 1903 he married Margaret Elizabeth Clark, daughter of the Reverend and Mrs. James Starr Clark, of English ancestry. Her father conducted a boys' school at Tivoli on the Hudson River where she was born. She was a graduate of Barnard College. Dr. Sumner and she met first while both were students at Columbia University, but two summers spent by her as a student at the Marine Biological Laboratory at Woods Hole played no small part in the outcome. The Woods Hole Laboratory has often been regarded as a highly successful matrimonial bureau for biologists; in this case, though not entirely responsible, it was an important factor in furthering the acquaintance and interest. In the "Life History" Sumner wrote: "The lapse of more than forty years finds me still married, and to the same woman." This, he thinks, may require some explanation in view of the present day trend away from permanent marriage and of his own highly individualistic character. The explanation is that she had been an exceptional partner. "Simple justice," he wrote, "requires an acknowledgment of the devoted help which she has rendered me throughout our life together," not in scientific collaboration but in "keeping a constant watch over my needs, mental and physical, and sparing my energies in every way. Any ambition which she may have had for self-expression on her own account she has largely submerged for the sake of my undertakings, which may all, to this extent, be regarded as our joint undertakings." She and three children, all married, survive him.

Also in 1903 he was appointed director of the laboratory of the United States Bureau of Fisheries at Woods Hole, a three months' summer appointment, renewable yearly. Three years later he resigned his teaching position and with his wife and child lived at Woods Hole continuously for the following five years, except for six months at the Naples Zoological Station in 1910. The change was made possible by creation of a temporary all-year position for him at the Bureau of Fisheries

laboratory in connection with the undertaking of a biological survey of the Woods Hole region.

Following the return from Naples in 1910 there was more than a year of uncertainty and anxiety as regarded the future, but appointment as naturalist on the Bureau of Fisheries exploring vessel, "Albatross," provided at least temporary respite. This appointment took Sumner back to California with the expectation of making long exploring trips. However, because of the condition of the ship and lack of funds for reconditioning, exploration was limited to a biological survey of San Francisco Bay. This position offered little promise for the future but it did bring him into association with the biologists of the University of California. After two years a suggestion by Dr. C. A. Kofoid led to correspondence and a meeting with Dr. W. E. Ritter, director of the then recently established Scripps Institution for Biological Research at La Jolla, California. The result of this meeting was acceptance by a committee, consisting of Dr. Ritter and other members of the biological faculty of the University of California of a project presented by Sumner for field and laboratory study of certain rodents and his appointment in 1913 as a permanent member of the staff of the Scripps Institution, where he remained until his death.

Turning to a survey and evaluation of Sumner's scientific work, it must be emphasized first that he was a tireless worker who found his greatest pleasure in research and that, as his publications indicate, the range of his interests was very wide. He said of himself that his mental attitude was that of a dilettante, turning from one problem to another as his interest changed. Probably very few besides himself would regard him as a dilettante.

His thesis was a descriptive study of certain aspects of fish embryology concerned with the problem of embryo formation. It was soon followed by a paper concerning the cell movements involved in fish embryo formation. This paper was his only contribution to experimental embryology. The experiments consisted in interfering with, or preventing, the cell movements occurring in certain regions in the course of development by inserting small glass needles through the cellular layers into the

yolk beneath them. The resulting modifications of development permitted certain conclusions. Effects on development of destruction of various areas were also determined and it was found that when the region in which the embryo normally appeared was destroyed another region could give rise to an embryo. This experimental study was carried on during his directorship of the Fisheries laboratory. These two papers played a very considerable role in modifying views concerning the method of formation of the vertebrate embryo.

At that time the question of Lamarckism versus Darwinism was still a subject of active discussion. This is the question, familiar to all biologists, whether characteristics acquired during the life of the individual are inherited and environment is directly concerned in determining the course of evolution, or whether environment is concerned only indirectly through natural selection of genetic, and therefore, inheritable, "chance" variations, that is, by survival of the "fittest" for particular environments? One result of Sumner's interest in this problem was an attempt to determine whether certain fish species showed any measurable differences which might be of selective value between survivors and non-survivors when subjected to various external toxic or lethal agents. With hundreds of measurements no definite differences were discovered, but much later one of Sumner's students did obtain more definite results. One of the agents used by Sumner with marine fishes was fresh water and questions of the osmotic relations of fishes to the aquatic medium, changes in internal salt content and the role of the gills and the external body surface in these relations led to extended investigations continuing through several years of the Woods Hole period and to several papers of very considerable interest.

Another line of investigation concerned with the problem of evolution, and particularly with the inheritance of acquired characters, was an attempt to determine whether keeping white mice at different temperatures from birth would produce heritable effects. This study was undertaken as incidental spare-time work during the Woods Hole biological survey but was continued for a number of years. It also involved a large number of measurements. The early results seemed to show positive inherited

effects of the different temperature environments, but later data were not entirely consistent and the work ended in uncertainty. In the "Life History" Sumner has pointed out the inadequacy of the technique of these experiments as regards control of the character and past history of the stocks of mice and of nutrition and has stated his belief, based on his later work with the deer mouse, *Peromyscus,* that it appears highly improbable that heredity played any part in the differences in the generation born of parents kept at high and at low temperatures. The chief result of these experiments involving thousands of measurements on some twenty-three hundred mice was, he believed, a valuable addition to his own experience.

As regards time and labor involved, the chief work of the Woods Hole years was the biological survey of the Woods Hole region with several coworkers and the preparation of the two-volume report. The survey, covering some four hundred and fifty "stations," involved an enormous amount of pure routine work and drudgery, dredging, taking bottom samples, recording temperature and salinity at each station, identifying the species taken or sending the material to specialists, and preparing distribution maps for the different species. Correlation of the data and the generalizations from them were undertaken by Sumner. Probably the most important result of the survey was the evidence that closely related species, differing only slightly morphologically show definite differences in distribution, which can often be clearly correlated with environmental differences. It appears that the morphological differences may themselves be of little or no significance, but are associated with, or are to be regarded as expressions of, physiological differences which determine the distribution.

The later biological survey of San Francisco Bay was essentially similar in character but the biological material was turned over to specialists and the data were published by them in separate papers. Sumner reported on the physical conditions in the bay. In discussing these two surveys and the resulting reports in the biographical sketch prepared for the National Academy of Sciences, to which he was elected in 1937, he has characterized them as yielding "relatively low grade ore," al-

though the work was not carelessly done and the methods, while not up to the standard of later oceanographic surveys, were fairly accurate.

The visit to the Naples Station in 1910 led to a study of the color changes in certain fishes, particularly in small flatfish, in relation to environment. He supplied these fishes with a variety of environments, some natural, some artificial, differing not only in color, but in pattern, that is, in distribution of different colors or shades, and kept photographic records of the resulting changes in color and pattern of the fishes. He found that they altered not only their color but the pattern of distribution of the pigment in relation to different environments with the general result that they became less visible. The alterations of pattern consisted in change in distribution of lighter and darker areas, not in copying of definite outlines or figures in the environment. This study ranks high among the various lines of investigation undertaken by Sumner and was so regarded by him. The published results aroused much interest, both scientific and unscientific and gave rise to some bizarre speculations by certain biologists and others. The work also provided a basis for highly interesting further research on color in fishes in relation to environment, begun some twenty years later.

The next important step in Sumner's career after the two years as naturalist on the "Albatross" was his appointment to the staff of the Scripps Institution and the beginning of seventeen years' study of the geographic races of the deer mouse, *Peromyscus*, in their relation to environment. These races occur in widely different habitats all over the United States and beyond its boundaries and show great differences in color and other characteristics. The differences in color of the different races were found in general such as to decrease the visibility of the animals in the environment in which they lived. Those living in light colored environments, such as many desert regions, or on white sand, are pale or virtually white; those living in regions of relatively high humidity with much vegetation and dark soil are dark in color. Collections by trapping of living animals were made on field trips, not only from many California localities, but from other states, even as far away as Florida. Some of his

amusing experiences during these collecting trips are recounted in the "Life History." Some observers of his trapping activities, finding that he was a member of the University of California were indignant that the state should permit such foolishness at the expense of the taxpayers. Others raised the question so often asked of the collecting biologist: "What is the use of all this?" One, an intelligent Hopi Indian, on whose ranch Sumner was trapping, inquired what he intended to do with the mice, and when informed that they were to be taken to California asked, "Why, ain't they got no mice in California?" Sumner found it no more and no less difficult to explain to him, than to the average white man, in some measure the purpose for which the mice were collected. The mice were brought to the Scripps Institution at La Jolla and bred there for many generations. Many thousands of measurements of color, length, area and weight were made. Results of this study published in numerous papers gave Sumner still wider scientific recognition. Only a few of the more important points can be mentioned here.

The different races bred in La Jolla environment did not show increasing resemblance to each other in successive generations. No evidence of inherited direct effects of environment appeared. According to Sumner, the different races appear to have arisen through accumulation of small differences, not essentially different from those between individuals of the same race. As regards color, this accumulation has probably occurred by natural selection of characteristics aiding in concealment of the animals. Many differences, for example, differences in proportions of the body and length of appendages, have no evident adaptive value but they may be morphological expressions of significant physiological differences. High humidity has been regarded by some biologists as directly favoring development of dark pigmentation in the individual. Sumner was at first inclined to accept this view but later became convinced that aerial humidity and soil moisture are chiefly effective only indirectly on the colors of the mouse races by their influence on soil color. The bare, or mostly bare, soil of arid regions is in general pale in color and the light colored animals are less visible there; in

damp regions the soil is darker and dark colored animals are less visible.

Also during the earlier years of the mouse studies Sumner thought that the Mendelian theory of inheritance was not adequate to account for the blending of characteristics when two races were crossed but came finally to accept the concepts of modern genetics, though, as he has admitted, somewhat reluctantly, partly because particulate theories, such as the gene theory, were repugnant to him as they are to some other biologists, and partly because he disliked what he viewed as attempts to get on the bandwagon of Mendelian theory. Chapter XVIII of the "Life History," dealing with the mouse research, is particularly interesting reading, both for biologists and others. One point as regards procedure in such investigations is worth noting. Measurements, particularly biological measurements, which are usually not exact in the physical sense and in which the personal equation is relatively large should not, Sumner believed, be left to assistants, with the investigator considering only the significant results which appear, but should be largely or entirely made by the investigator himself, both in the interests of greater accuracy and in order that he may realize more clearly just what his data mean.

One paragraph from Chapter XVIII of the "Life History," calling attention to some of the unsolved problems of evolution with particular reference to the *Peromyscus* study deserves quotation in full. "Of a certainty, we still know very little about the process of evolution in its details, however sure we may be (and have a right to be) of its actuality. With all our faith in the 'survival of the fittest,' we are rarely able to point out the particular elements of 'fitness' that have enabled a particular type to survive. Nor do we know precisely the hazards which they survive. Why, for example, is one species of *Peromyscus* restricted to three of our southeastern states, while another is so widely distributed that 'it is probable that a line, or several lines, could be drawn from Labrador to Alaska and thence to southern Mexico throughout which not a single square mile is not inhabited by some form of this species' (Osgood)? Why are some species of races restricted to woodland, others to open

country, others to bare rocks, etc.? Because they are especially 'adapted' to these various habitats, we say. Of course, but has anyone taken the trouble to find out what this 'adaptedness' consists in? Presumably we shall be able, in time, to answer some of these questions, but until then much of our discussion of adaptation must remain airy speculation."

The *Peromyscus* researches would undoubtedly have continued throughout Sumner's life if the change in purpose and title of the Scripps Institution for Biological Research into the Scripps Institution of Oceanography had not made abandonment of the entire program virtually obligatory after three years of support by the Carnegie Institution. The abandonment of this research program was deeply regretted by Sumner and was regarded by his biological colleagues as a very great mistake, "a colossal blunder," as one of them has put it. Sumner had made the field his own, had attained results of very great interest and importance as regards inheritance and evolution and there was every promise of further results of no less interest and value. However, in spite of his deep disappointment no time was lost in bemoaning fate. He turned again to the problems of color and its changes in fishes and continued work in that field until his death, this line of investigation being regarded by the directors of the Scripps Institution as within the scope of its function.

He discovered that in addition to the relatively rapid and readily reversible changes of color and pigment pattern in relation to environment, which are due to altered distribution of pigment granules already present in cells of the integument, the total amount of pigment present may undergo increase or decrease more slowly, also in relation to environment and also with the result of decreasing visibility of the fish. These studies involved chemical determinations of amounts of pigment and were carried on with the aid of coworkers. The determinations demonstrated that the dark pigment, melanin, increased in certain fish species kept on increasingly dark backgrounds with uniform intensity of illumination. In the same species the white pigment, guanin, increased in amount when the fishes were kept on increasingly light backgrounds with uniform

illumination. These experiments appear to have established beyond doubt that the factor effective in determining the reaction is the albedo, that is, the ratio between the light reflected from the surface of the background and the incident light by which it is illuminated. Sumner has pointed out that this type of reaction seems to have something in common with the ability of the human being to recognize shades correctly under different lighting conditions. However, neither in the fish nor in man is anything known concerning the mechanism of such reactions.

The conclusion that so-called protective coloration actually protects has been disputed in recent years by various biologists, but Sumner's researches have demonstrated conclusively by actual experiment that protective color does protect. Fishes so pigmented that they were less distinctively visible to the human eye in a given environment were less frequently taken by predatory birds and also by larger predatory fishes than other individuals of the same species so pigmented that they were more clearly visible.

Studies of fish metabolism in relation to temperature were also carried on with coworkers. Sumner was interested in the adaptation or acclimation to high temperatures, in nature, to life in warm springs at temperatures which are lethal to unacclimated individuals. Data of interest concerning the differences in oxygen consumption at the different temperatures and the changes during acclimation were obtained.

In a discussion of "The Naturalist as a Social Phenomenon" at a symposium some years ago (American Naturalist, 74, 1940) Sumner raised the interesting question whether he himself or others would continue research if there were no possibility of publication or other method of communication of the results. He believed that the answer would be an almost unanimous negative, including his own. Moreover, he maintained that even with anonymous publication there would be comparatively little interest in research. This attitude of mind he regarded, not as evidence of selfishness or due to personal vanity but rather as indicating that even the scientific man is a gregarious animal and obtains satisfaction "from the approbation and sympathetic understanding" of his colleagues.

He believed that all science, even the "purest," is completely justified by its value to human life, quite apart from any practical application. No matter how small the contribution of the individual may be, he is heading in the right direction. "But we must recognize the existence of various standards of value other than economic or practical ones. I merely insist upon applying to science the same standards as those which we apply to music, literature, art or religion, namely, their contribution to the life of the whole man, taken in the broadest possible sense. * * * Science is not the handmaid of industry, nor is it a mere intellectual pastime. It is a quest for the facts and principles upon which to erect a true philosophy of life" (Life History, p. 262). Many of the so-called benefits of science, particularly those which have made it possible for larger numbers of human beings to live in a given restricted area he regarded as something other than unmixed benefits to man. One does not know his reaction to the military applications of atomic research.

Mention of some of his many interests outside the fields of his researches serves to throw further light on his personality. He had a deep appreciation of, and love for, undespoiled scenic nature and was particularly attracted to the so-called desert regions of Southern California, Arizona, and New Mexico. The writer shared his feeling and was privileged to be his companion on various trips into these regions. No better companion could be desired. He was an ardent conservationist, was a member of various conservation organizations and wrote and spoke in support of his views. He deeply deplored the waste of natural resources and the substitution of ugliness for natural beauty. The "booster" type was anathema to him. However, he has pointed out that the efforts of conservationists are largely futile. They, like other "reformers," are amateurs. "Their opponents are professionals. How can a few zoologists, botanists and nature-lovers, innocent of the game of politics, make any headway against such eminently practical people as lumbermen, real-estate promoters, cattle and sheep raisers, water-power magnates, sportsmen and ammunition dealers—groups united in the common enterprise of destroying our wild life and our scenic beauty?" (Life History, pp. 225-6).

Closely associated with his views concerning conservation was his firm belief in the necessity of birth control. He held that the United States as well as the world in general is overpopulated and that, without restriction of increase, life must become less worth living. He was no less active against the antivivisectionists, but considered the medical defense of vivisection unjustified or actually disingenuous. He wrote in the "Life History": "Every biologist knows that a large part of the work conducted in our laboratories of physiology and experimental biology has only the remotest relation to questions of health and disease. They (?) relate to fundamental problems or animal life or plant life, and the investigators themselves are seldom thinking of any practical applications of their discoveries. Why must we tell the voters that medical progress is the real object of all such work? Why not try to give them a glimpse into the amazing world of life processes which has been revealed during the past hundred years and point out to them—though this would hardly be necessary—that these things could only have been discovered through the study of living animals, *while living*." For several years he was a member of the Advisory Board of the Society for the Legalization of Euthanasia. That he stood for freedom of speech and civil liberties goes without saying. Various titles in his bibliography indicate the wide range of his interests. Some time before his death he had a part in establishing a discussion club at La Jolla, including not only members of the Scripps Institution staff but others in the town who were interested. Since his death this has been named the Sumner Club in his memory.

His discussion of "that great welter of contradictory thoughts and emotions which is suggested by the word 'religion'" in Chapter XXIII of the "Life History" reveals his independence of thought and his intellectual honesty as clearly, perhaps, as anything he has ever written. It seems to the present writer a remarkably fine example of straight thinking in a highly confused and controversial field. Concerning his own views, only the following sentences need be quoted. He wrote: "I have long been intrigued by the notion of a creative principle which is itself undergoing evolution. This great chaotic universe,

wasteful, cruel and imperfect in countless ways, is so because both it and its maker are in the process of making. Creation is a vast process of trial and error, and the creator itself cannot foresee a result prior to any experience of this. Such a view divests the creative agent of some of its omnipotence, to be sure, but it seems to me the only alternative to a view which makes cruelty and injustice basic in the organization of the universe."

Sumner's serious concern with many problems of science and social life was balanced by a deeply, though quietly humorous vein which often found expression in the most unexpected ways and was much appreciated by those who knew him. It is evident in his discussions of P. D., the Personal Demon, which he, like most of us, sometimes felt was responsible when things consistently "went wrong," not only in the ordinary concerns of life, but in experimental science. A paper in the Scientific Monthly, entitled, "The Philosophical Basis of Pediatrics," is concerned, not with pediatrics in the usual sense, but with P. D., the Personal Demon. Referring to P. D. on page 292 of the "Life History," he says: "I shall leave to my philosophically minded readers the task of identifying this Personal Demon (The 'P. D.', as some of us familiarly call him in our laboratory) with the famed 'Uncertainty Principle' of Heisenberg and his legion of followers. I am disposed to feel that my discovery, which surely antedates Heisenberg's by several decades, entitles me to full priority in the matter! All that Heisenberg seems to have added is the mathematical garnishing."

Some of Sumner's colleagues have been inclined to regard him as a pessimist. However, what may have seemed to them to be pessimism was evidently nothing more than the dislike of one as honest as he was for much that he saw in the world, even the scientific world, about him. That dislike very commonly expressed itself humorously rather than in bitterness or discouragement. His interest and activity in relation to conservation and other social problems is a sufficient answer to those who considered him pessimistic. Moreover, no thoroughgoing pessimist would or could have devoted himself so whole-heartedly and tirelessly to research in "pure" science as he did. His absolute

scientific integrity has influenced and helped many younger workers, both at the Scripps Institution and elsewhere. The writer of this memoir knew him, not only as a zoologist, but as a good companion and, since his death on September 6, 1945, has felt keenly the loss of a highly valued friend with an extremely interesting mind.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Amer. Fish Cult. = American Fish Culturist.
Amer. Jour. Physiol. = American Journal of Physiology.
Amer. Nat. = American Naturalist.
Anat. Anz. = Anatomischer Anzeiger.
Arch. f. Ent. Mech. = Archiv für Entwickelungsmechanik.
Biol. Bull. = Biological Bulletin.
Bull. Bur. Fish. = Bulletin, Bureau of Fisheries.
Bull. Scripps Inst. = Bulletin, Scripps Institution.
Jour. Exper. Zool. = Journal of Experimental Zoology.
Jour. Hered. = Journal of Heredity.
Jour. Mammal. = Journal of Mammalogy.
Jour. Phil. Psychol. Sci. Methods = Journal of Philosophy, Psychology and Scientific Methods.
Mem. N. Y. Acad. Sci. = Memoirs, New York Academy of Sciences.
Physiol. Zool. = Physiological Zoology.
Proc. Nat. Acad. Sci. = Proceedings, National Academy of Sciences.
Psychol.. Rev. = Psychological Review.
Sci. Mo. = Scientific Monthly.
Trans. Amer. Fish. Soc. = Transactions, American Fisheries Society.
Trans. N. Y. Acad. Sci. = Transactions, New York Academy of Sciences.
Trans. San Diego Soc. Nat. Hist. = Transactions, San Diego Society of Natural History.
Univ. Calif. Publ. Zool. = University of California Publications in Zoology.
Zool. Soc. Bull. = Zoological Society Bulletin.

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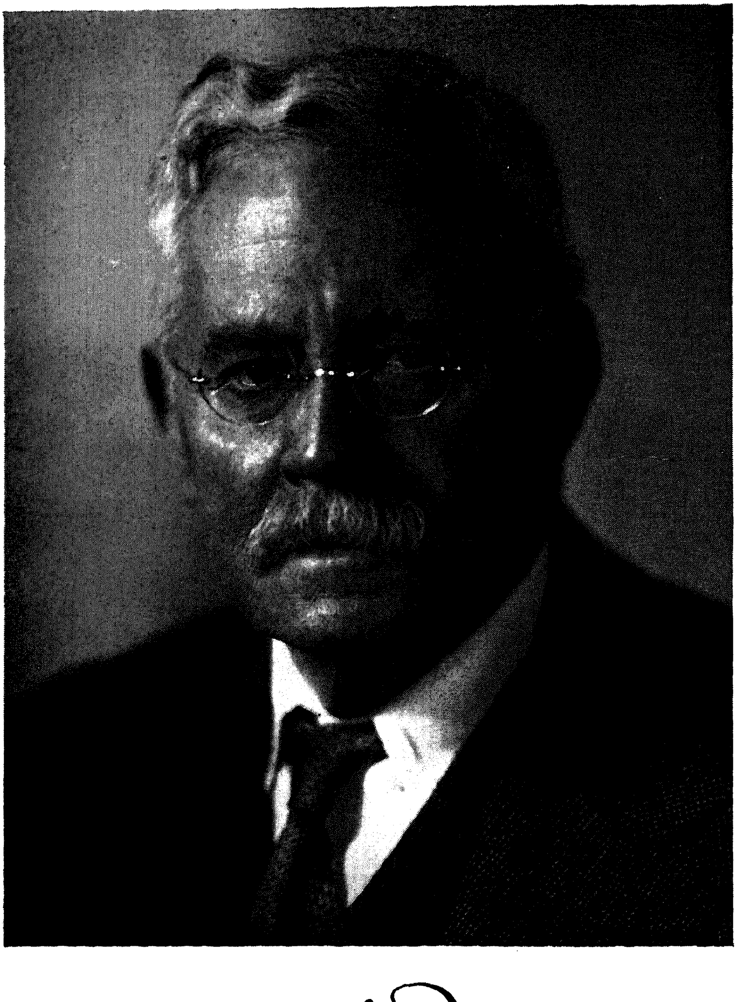

W. B. Scott

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WILLIAM BERRYMAN SCOTT

1858–1947

BY

G. G. SIMPSON

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WILLIAM BERRYMAN SCOTT

1858–1947

BY G. G. SIMPSON

1. *Life*

A great-great-great-grandson of Benjamin Franklin, grandson of Sarah Bache, great-grandson of Catherine Wistar (sister of Caspar), grandson of Charles Hodge (for 56 years professor in Princeton Theological Seminary), and son of William McKendree Scott (a graduate of that Seminary), might be expected to have close associations with Philadelphia and with Princeton. It seems almost anomalous that William Berryman Scott was born in Cincinnati, Ohio (on 12 February 1858), but in 1861 his family moved to Princeton, which was to be his home during most of his life. His father died in December of that year. Young William Berryman was raised by his beloved mother, Mary Elizabeth (Hodge) Scott, and her father, Charles Hodge, whom his grandson "held in a love and reverence that are quite impossible to describe."

Scott was frail and sickly in childhood. He had little companionship in his own age group and his adult associates were kindly but sober and deeply religious. His childhood reading ran to theology, philosophy, and the classics. In college he took some part in athletics and he formed wider and more appropriate friendships, but his sedate, withdrawn childhood left some permanent traces in his character. His early education was also affected. He studied with his mother, with tutors, and in a series of schools most of which he judged worthless in retrospect. His inadequate preparation did not, however, prevent entrance to the College of New Jersey (now Princeton University) at the age of 15, in 1873. Native ability soon compensated for an initial handicap, and he soon went to the head of his class and was tied for first place at graduation.

Precociously mature, quiet, and sensitive, the young student (by his own later statement) was not popular outside of a restricted circle of intimates. Outstanding among these were his classmates "Sally" Speir (Francis Speir, Jr.) and "Polly"

Osborn (Henry Fairfield Osborn), both of whom became lifelong friends. To these contemporaries and to his family, Scott, himself, was "Wick."

The future author of "The theory of evolution, with special reference to the evidence upon which it is founded," had assumed as a boy that he would enter the clergy and had acquired from his revered grandfather a violent antipathy toward all evolutionary ideas. In his junior year he took a course in geology under Guyot, but he was more fascinated by such courses as psychology under McCosh (then president of the College) and literature under Murray. The turning point, often recalled in later years, was marked by an idle remark during the examination period of his junior year, in June, 1876. Scott, Osborn, and Speir were lying on a canal bank after a swim, trying to read Paley but finding the day too balmy for natural theology. His daydreaming inspired Scott to say, "Fellows! I have just been reading in an old Harper's an account of a Yale expedition to the Far West in search of fossils; why can't we get up something of the kind?" The query was not meant seriously, but Osborn and Speir acclaimed it with, "We can. Let's do it!" That idle moment completely changed the careers of Scott and of Osborn and had a profound influence on the history of paleontology. There is a certain irony in the fact that the inspiration came from O. C. Marsh (his was the expedition related in Harper's Magazine), whom Scott later "came nearer to hating . . . than any other human being."

The projected expedition materialized after the three friends were graduated in 1877. It was a grand affair led by Dr. Brackett, professor of physics, with General Kargé to lead the Indian fighting (which happily did not occur), eighteen students, and two cook-teamsters. After a journey through Colorado, which appeared to the would-be bone diggers as mere aimless wandering, the professor and the general quarreled. This had the result, most fortunate as it proved, that the general left the main party and, with Scott, Osborn, and Speir went to Fort Bridger, Wyoming. In that region they made a good collection of early Eocene mammals, including a skull of *Uintatherium*, which Dr. Guyot had especially wanted and which was large

enough and grotesque enough to attract wide attention in Princeton.

Speir eventually became an attorney, although he long retained a collector's and amateur's interest in fossils. The professional fate of Scott and of Osborn was sealed by their early collecting and work on the resulting specimens. It became inevitable that they should be geologists, biologists, or (as finally eventuated) a combination of the two, that is, paleontologists. A graduate year at Princeton, 1877-78, was devoted to studying anatomy, among other things, and especially to the enthusiastic and, at first, somewhat amateurish study of their own collection.

At this time the great figures in paleontology in this country were Joseph Leidy and E. D. Cope of Philadelphia and O. C. Marsh of New Haven. Cope and Marsh were bitterly antagonistic and the Cope-Marsh feud was getting into full swing. Leidy, disgusted with this affair, was withdrawing from paleontology into the more peaceful fields of biological research. After an initial rebuff caused, perhaps, by his suspicion that they might have some connection with Marsh, Cope became cordial and helpful to Scott and Osborn. They maintained warm friendship with him to the end of his life some twenty years later. They were inevitably drawn into Cope's war with Marsh, as the years went by, and they staunchly supported Cope's side. In memory, they kept this factional bitterness alive long after the original feudists had disappeared from the scene. Scott always maintained that some day he would write and publish to the world the full story of Marsh's perfidy and other failings, but fortunately he never carried out this threat, which could, indeed, have been countered by evidence that Cope was human, too. The young students also met and greatly admired Leidy, who gave them considerable assistance in their first venture into paleontological research.

In the summer of 1878 there was another Princeton expedition, this time on a more modest scale and with a more definite purpose. "The triumvirate," Scott, Osborn, and Speir, returned to the Bridger Basin where they obtained another good collection of fossil mammals as well as a fine lot of Eocene fish from the now famous Green River Shales.

That autumn both Scott and Osborn went to Europe to complete their advanced studies, the usual procedure for higher professional education in that day. Scott landed in London with no concrete idea as to what, where, or with whom he was going to study, but he carried a letter from Leidy to Huxley, and Huxley took him in hand and helped to direct his further development. After attending Huxley's lectures at the Royal College of Science and working in his laboratory, Scott moved to Cambridge where (with Osborn) he studied embryology under Balfour. Thence he went to Germany, where he perfected his knowledge of German and worked with Gegenbaur at Heidelberg. There he wrote a dissertation on the embryology of the lamprey, *Petromyzon,* and was made a Doctor of Philosophy in 1880.

Back in Princeton, President McCosh was energetically trying to improve and broaden instruction at the College and in 1880 he called to the faculty nine instructors to whom he referred as "my bright young men." Two of these were Scott and Osborn, both engaged to strengthen the teaching of science. Their interests and training had been almost identical, but academic needs turned Scott to the teaching of geology and Osborn to comparative anatomy. Both became vertebrate paleontologists, but this divergence in their early teaching left a mark. Scott added to his biological training a more evident geological background, and this is still noticeable in Princeton paleontology. Osborn's more purely biological outlook long characterized the Columbia school.

Scott served actively on the Princeton faculty for just fifty years, 1880-1930, and after his retirement he continued to work mainly at Princeton to the end of his long life. In all, he was associated with the College and the University (as it became in 1896) for seventy-four years, more than a third of the whole history of an institution which is accounted venerable in America and which celebrated its bicentennial in the year of Scott's death. He was promoted to full professorship in 1884 at what now seems the startlingly early age of 26. He was the first chairman of the Department of Geology, established in 1904, and served in that capacity until his retirement in 1930.

During this period he guided and, when necessary, fought for the improvement and expansion of the department, which grew from three to thirteen members under his leadership.

Scott considered teaching as his most significant work, and there is evidence that his students found him an inspiring and successful teacher. John Grier Hibben, who was in Scott's first class and who later became president of the University, wrote to Scott in retrospect: "You gave us a new view of scholarship as an adventure into the unknown and great world of knowledge. You had the spirit of an explorer returning from his absorbing quest and you were able by your enthusiasm to impart that spirit in large measure to us." Fifty years in the classroom did not eliminate that enthusiasm, as witness the student reporter who wrote when Scott retired, "We cannot view with equanimity a world bereft of some of his lectures such as the 'Eruption of Vesuvius' or the even more popular 'Johnstown Flood'."

Scott confined his teaching to undergraduates. He did not give professional training in geology, but he built up a department which became signally successful in such training. He founded no school of paleontological practice or theory, but under his younger associate, W. J. Sinclair, and under Sinclair's student, G. L. Jepsen, Princeton continued to develop as a major center for both teaching and research in vertebrate paleontology. Within this science, Scott's important direct influence was rather through his activity in professional societies and through his publications than through teaching.

In 1883, when his salary had reached the substantial figure of $2,500, Scott married his boyhood sweetheart, Alice Post. The marriage was singularly happy and was clouded only by having two children, Anne and Hugh, die in infancy, and another, their first child, Charles Hodge Scott II, die before them (in 1926) at the age of 41. Four daughters, Adaline, Angelina, Mary, and Sarah survive, as does Mrs. Scott.

In his early years on the Princeton faculty, Scott spent eight summers in the field, building up fresh collections without which vertebrate paleontology stagnates or becomes an armchair science. The first of these expeditions, in 1882, worked

mainly in the White River Badlands and began from that extraordinary deposit the accumulation of Oligocene mammals which, with rich additions by Sinclair and others, has become an outstanding feature of the Princeton collections. Perhaps at this time was born an ambition to revise the whole of that great mammalian fauna, a plan carried to completion almost sixty years later. In 1884 Scott and his party visited the Big Horn Basin of Wyoming in search of early Eocene mammals, but this was, in Scott's own words, "a complete failure." This cannot be said of any other Princeton expedition for fossil vertebrates, and the stigma, if any existed, was fully wiped out by later work under Sinclair and Jepsen which has amassed, mainly from the Big Horn Basin, one of the greatest collections of early Eocene mammals in the world.

In 1885 and 1886 Scott returned to the Bridger beds, where he had worked with Osborn and Speir in 1877 and 1878. In 1889 he collected from the John Day formation in Oregon. In 1890 a brief visit was made to South Dakota and in 1891 Scott collected in the vicinity of White Sulphur Springs in Montana. On all of these expeditions Scott took along parties of students or recent graduates and their contributions paid for the expenses of the work.

In 1893, John Bell Hatcher, one of the greatest of fossil collectors, left Marsh's employ and came to Princeton to work under Scott. That summer Scott and Hatcher returned to the White River Badlands and had phenomenal success. The results (quoting Scott) were "incomparably greater in amount" than any from Scott's earlier parties and reflected "the difference between professional and amateur collecting." Scott felt that there was no real need for him, put the party in Hatcher's hands, and returned home early. Scott never again collected a fossil, although he was only 35 when he made this renunciation of field work and although he continued to be a frequent traveler. Hatcher made other large western collections for Princeton, and in 1896-99 conducted the Princeton Patagonian Expeditions, which occupied Scott's research and editorial efforts for many years. In 1900 Hatcher went to the Carnegie Museum in Pittsburgh and he died in 1904 at the early age of 43. After

Hatcher, the continuously successful Princeton fossil vertebrate collecting campaigns were directed or conducted, in the main, first by Sinclair and more recently by Jepsen.

The facts that Scott still considered himself an amateur after ten collecting expeditions and that he gladly turned over this activity to others as soon as opportunity presented, demonstrate his own judgment that field work was not his strong point and perhaps also reflect a certain distaste for this physical activity, even more strenuous then than now. He certainly appreciated the picturesque aspects of life in the field, amusing incidents of which were often repeated in later years and occupy much space in his autobiography. He must, however, have lacked the real love of life in the open and the burning passion for the fossil chase that activate so many of his colleagues. Nevertheless, his field experience gave him a touch for geological reality which is missing in those paleontologists whose experience has been mainly indoors, and it laid the basis for a collection that has become one of the major assets of American vertebrate paleontology.

Even though he may have had mixed feelings about camping, tramping, and digging, Scott surely loved to travel, to see new places and to make and renew friendships. London, the only large city in which he ever lived, always retained a special attraction for him. His friendships with British colleagues were also among the most treasured things in his life. One of the few drawbacks of his long and unusually placid, happy life was the sorrow caused by the death before him of most of these old friends.

Scott learned to speak German fluently, and his student days in Germany left him with a strong admiration for many things in that country. The German universities were then at their height and Scott considered them the best in the world. (He later recognized and deplored their decline from this peak.) He found most Germans friendly and admired, especially, their widespread respect for learning, science, and art. He noted, however, "the hard egoism and envious disposition which characterized so many Germans." He highly esteemed such colleagues as Zittel, Schlosser, and, especially, E. Fraas, but he

seldom spoke of German friends with the same warmth as of the British. He noted and detested the influence of the army in Germany and foresaw where it would lead. The first World War so upset him that he could not work outside of routine. He was eager to have the United States enter what he considered a crusade against Prussian militarism for the benefit of what was good in Germany as well as for the peace of the world. Even this degree of faith in the Germans was shaken by the second war.

One of the highlights of Scott's life was a long journey in 1900 which took him to Germany and England again and then to the Argentine by way of Portugal and Brazil. The main purpose of these travels was to study and photograph type specimens of fossil mammals from Patagonia, for use in research on Hatcher's collections. Paleontology in the Argentine was then very active but bitter in a way too reminiscent of the days of Cope and Marsh in North America. Moreno, Burmeister, and Ameghino were carrying on a three-cornered feud which stopped at nothing in the way of vituperation or even, at times, more direct injury. Scott managed to steer his way with consummate diplomacy and was able to examine not only the official collections, in the Museo de La Plata, then controlled by Moreno, but also the Ameghino collection (considerably superior in the field of interest to Scott), then still private property. Ameghino had acquired a reputation for being secretive regarding his specimens, but Scott's open and friendly approach gave him full access to all of these and even free permission to figure them in future publications. This outcome speaks well for both men and establishes the fact that any fault that may have existed in Ameghino's attitude was provoked by his enemies.

This journey and Scott's extensive publications on Patagonian fossils gave rise to a widespread impression that he had explored Patagonia. Three of Scott's outstanding characteristics, integrity, modesty, and humor, are reflected in his later remarks that some of the awards made to him must have been based on this mistake and that he was the only man who had been honored for exploration in Patagonia without the preliminary formality of visiting that region.

Others among Scott's many travels that stood out particularly in his memory were the trip to the South African meeting of the British Association for the Advancement of Science in 1905, involving a circumnavigation of Africa as well as wide land excursions, and a cruise to the Canal Zone in 1911. At various times he also visited Italy, Spain, Switzerland, Cuba, and many other parts of the world, with Australia and the Far East as the major exceptions. During the long period after he had an established reputation and while he retained full physical vigor, he played an unusually active part in national and international scientific affairs. In the latter field, he was a frequent delegate to the international zoological and geological congresses and to celebrations of foreign universities and societies. In addition to the British Association, he was a member of the Geological, Zoological, and Linnaean Societies of London.

Scott's election to the National Academy of Sciences came in 1906, when he was 48 years of age. This will not seem a very late election to many members of the Academy and forty-one years as a member is exceptional, but Scott indicated, with a touch of naïveté not entirely characteristic of him, that he considered his election long deferred. He ascribed this delay to the enmity of Marsh, who died seven years before Scott's election but who had been a dominating figure in the Academy during the years when Scott was rising to eminence. Scott was more active in the American Philosophical Society, where he had a truly remarkable record. He was elected in 1886 at the age of 28 and was a member for sixty-one years. Until the last few years of his life he missed few meetings except during his trips abroad. He was president of the Society from 1918 to 1925. He was also active in the Paleontological Society, of which he was president in 1911, and the Geological Society of America, president in 1925. He took a friendly interest in the recent organization of the Society of Vertebrate Paleontology, but left the active work of that society to younger colleagues. Among other professional organizations to which he belonged were the American Academy of Arts and Sciences, the Academy of Natural Sciences of Philadelphia, the Academies of Sciences

of New York and of Washington, and the American Association for the Advancement of Science.

Recognition of Scott's work included honorary degrees from the University of Pennsylvania (1906), Harvard (1909), Oxford (1912), and Princeton (1930). The National Academy of Sciences bestowed on him both the Mary Clark Thompson Medal (1930) and the Daniel Giraud Elliot Medal (1940). He also received the Wollaston Medal (1910) of the Geological Society of London, the E. K. Kane Medal (1905) of the Geographical Society of Philadelphia, the F. V. Hayden Medal (1926) of the Academy of Natural Sciences of Philadelphia, the R. A. Penrose Medal (1939) of the Geological Society of America, and the Walker Grand Prize (1934) of the Boston Society of Natural History.

After his retirement in 1930, Scott applied himself with even closer devotion to research, but his other professional activities inevitably tended to become more restricted. His time at home was divided between Princeton and Cataumet (on Cape Cod) and he was a frequent and welcome visitor at the American Museum of Natural History, the Museum of Comparative Zoology at Harvard, and other institutions with collections of interest to him. During this period he undertook and completed his revision of the White River fauna, and followed this with an almost equally ambitious plan to revise the late Eocene Uinta fauna. In spite of his sickly childhood, he retained unusual physical and mental vigor to an advanced age. His powers began visibly to fail only in the last few years of his life and he was at work on the Uinta monograph up to two days before his death, which occurred at Princeton in his ninetieth year on 29 March, 1947.

This brief and formal account of Scott's life has perhaps failed to portray its subject in the round and in full color. If so, the proper corrective is to read Scott's autobiography (1939, listed in the bibliography, below). There he reveals his own admirable character more fully, consciously and unconsciously, than could be achieved by a biographical memoir. He did not have the overpowering presence of Osborn and certainly not the slightly rakish air of some of his other colleagues, but he was

neither weak nor prim. He never wholly lost the reserved manner developed during his boyhood and he insisted on gentlemanly conduct in himself and in others, but he was flatly outspoken on matters of principle and was a charming conversationalist given to delightful sallies of rather dry, always kindly and never improper humor.

It is one result of his long life that most of those who can now write of Scott were separated from him by one or two generations. To us he became during his lifetime an almost fabulous link with the heroic past. The young and even the middle-aged Scott tends to elude us, except as he could be glimpsed through his own phenomenal memory and the introspection of his later years. As we knew him and as he revealed himself, he was characterized by mild but steadfast character, by unflinching integrity, by unassuming worth, and by "a mind at leisure from itself" (as he quoted on his eightieth birthday). His life was a masterpiece, long and full of happiness and of achievement.

(NOTE: This account of Scott's life has drawn heavily on his autobiography, published by the Princeton University Press. Some information has also kindly been supplied by Dr. G. L. Jepsen.)

2. *Works*

Throughout his career, the great bulk of Scott's published works consisted of descriptive studies of fossil mammals. This generalization fails, however, to convey an adequate idea either of the development or of the full scope of his research. This work was marked by common themes and by considerable continuity, but it can be divided into four intergrading periods. The earliest period began with his first technical publication in 1878 and lasted, approximately, through the 1880's. These were more or less the 20th to 30th years of Scott's life, and the important output of these years demonstrates the fact that Scott not only remained active to a great age but was also precocious. In this period Scott published his few embryological studies, products of his graduate work, and the first of many monographs on fossil mammals. A number of these papers were written jointly with Osborn, who was also at Princeton during

this period. Osborn went to New York in 1891 and collaboration then ceased although friendship continued.

Scott's second period covered roughly the decade of the 1890's and the 30's of Scott's life. It was (in this reviewer's opinion) the time of his greatest powers as a research student, characterized by a series of monographs of highest value and also by brief but searching discussions of evolutionary theory. The third and longest period, from around 1900 until the early thirties, was dominated by one great project, the Patagonian memoirs, and by the production of several textbooks. There was increasing maturity and no diminution of vigor in his work, but there was some narrowing of its breadth or depth, except as wider interests were reflected in compilation of his books. The last period, in the declining years of his eighth and ninth decades, were devoted almost exclusively to two projects, the White River monograph and the similar but uncompleted Uinta monograph.

Among Scott's outstanding contributions to the study of evolution were two memoirs, both published in 1891, the first on *Poebrotherium* and the second on *Mesohippus* and *Leptomeryx*. Besides the descriptive parts of these papers, the first included a list of what Scott considered the most important questions regarding evolution and the second attempted to answer these questions from the paleontological point of view. These treatments are of such exceptional interest for the history of paleontology and biology that it is worth while to summarize them here. The main questions and the gist of Scott's answers were as follows:

Regarding the "mode" of evolution, by which Scott meant the so-called "laws" or morphogenetic principles of evolution:

1. Are polytypic genera monophyletic or polyphyletic in origin? The genetical genera of taxonomic theory are monophyletic by definition. The morphological genera of practice are probably often of polyphyletic origin because of the widespread incidence of parallelism and convergence.

2 and 3. How are parallelism and convergence possible and to what extent have they occurred? Parallelism and conver-

gence, which are different degrees of the same phenomenon, are common results of the modification of different groups to achieve similar evolutionary ends. Both are extremely widespread in the historical record and they demand close attention to morphological differences if that record is to be read correctly.

4. Can a structure which has once been lost ever be regained? Paleontology cannot give a conclusive answer; it is probable that such a process can occur but rarely does.

5. Does evolution tend to move steadily in one direction? This seems to be normal, but not quite universal, in mammals. The generalization may not be valid for lower forms of life.

6. In higher animals, does advancing differentiation always involve reduction in numbers of parts? It normally does so. Reduplication can theoretically occur but is exceptional.

7. Does such reduction always occur in the same way in different lines? The mode of reduction is generally very uniform, in mammals, at least.

8. (As an example of many questions of a more special character—) What effects follow by mechanical necessity from great increase or decrease in body size? (Scott specified a number of such effects and showed them to be quite general.)

This really extraordinary list shows how progressive Scott was at that time and how much more morphogenetic theory owes to him than is now commonly realized. Although none of these points was, or was claimed to be, completely original with Scott, he is here seen to have discussed "Dollo's Law" (4) before Dollo, "Williston's Law" (6) before Williston, graviportal adaptation (8) before Osborn, orthogenesis (5) before Haacke or Eimer (indeed, before the term was invented), etc.

On the broader problem of the causes of evolution, which he called "factors" as opposed to "modes," Scott strongly criticized Weissmannian neo-Darwinism and took a definitely, although not extremely, neo-Lamarckian position. In a critical review of Bateson's work, Scott later (1894) maintained that random and discontinuous variations (i.e., very nearly mutations in the later genetical sense) have little to do with evolution, which normally proceeds by continuous and oriented change (mutation

in the original sense of Waagen, not in that of the later geneticists).

It is characteristic of this period of Scott's work that these broad and profound theoretical discussions were the outcome of and were appended to descriptions of particular fossils. Most of his monographs of various groups and genera were considerably more than mere description, important as were the descriptions as accurate, factual additions to knowledge. Among many other examples of major studies that oriented morphology against a broad background of phylogeny and theory may be mentioned such publications as his revision of the oreodonts (1890), several basic studies of the creodonts (e.g. 1888, 1892), revisions of the Uinta (1889) and Deep River (1893) faunas, and monographs on *Dinictis* (1889), *Ancodus* (1894), *Hyaenodon* (1894), *Protoceras* (1895), *Elotherium* (1898), and Eocene selenodont artiodactyls (1899).

After Hatcher's highly successful expeditions to Patagonia, Scott assumed the long and arduous task of editing the whole series of reports and writing some of those on Miocene (Santa Cruz) mammals. This work was begun in 1900 and continued, with an interruption during the first World War, until 1932 when the last of fifteen (nominally eight) luxurious volumes was issued. Aside from research in Princeton, it involved for Scott the voyage to the Argentine mentioned above and also several trips to Germany, where the earlier volumes were printed. Scott's own contributions were more narrowly morphological and taxonomic than much of his earlier work, but they were outstandingly good within their scope. In addition to their great and permanent factual value, they brought order and reason into an important field of study that was in a really chaotic condition when Scott entered it.

As an outgrowth of his teaching, Scott had written a textbook of geology, first published in 1897, and during the period of work on the Patagonian reports he twice thoroughly revised and expanded this (1907, 1932). During this period he also published a textbook of physiography (1922) and a book on evolution (1917), in which he stressed the evidence for the reality of organic evolution more than the theoretical principles

as to how and why evolution has occurred. A more direct outcome of his personal research and perhaps the most valuable of all his books was "A history of land mammals in the Western Hemisphere" (1913), in which he summarized all that was known of the history, characteristics, phylogeny, and distribution of mammals in North and South America. A revised edition of this book, issued in 1937, was so completely rewritten and brought up to date (with help from G. L. Jepsen and others) that it might be considered a new production. This work is truly a paleontological classic and is one of the most valuable text and reference books available to the mammalogist, paleo- or neo-. It has one peculiarity: Scott intended it as a popular book, although it is commonly considered far too technical for the general reader, and he felt that for this audience it was preferable to take up the history in the reverse of the usual order, starting with the recent and more familiar and working backward through time. This peculiarity, which is mentioned for its interest regarding Scott's work and personality and not as a criticism, was retained in the revision, over the protests of some of Scott's colleagues (but with the approval of some others).

In 1934, in his seventy-seventh year, Scott undertook another major research task, the revision of the White River fauna. With the assistance of Scott's successor, Jepsen, and with two of the smaller sections written by A. E. Wood, this was completed and was published in five quarto parts by the American Philosophical Society, 1936-1941. One more smaller study, on the Duchesne River fauna, was completed and published under the same auspices (1945), but another major project, on the Uinta fauna, was left unfinished.

There is no doubt that Scott will occupy a permanent and high place in the history of his science. He and Osborn dominated vertebrate paleontology in America, one might say in the world, as Marsh and Cope had in the preceding generation. The parallel can be pushed a little further, for Scott and Marsh were both at their best in dealing with practical and theoretical morphology, while Osborn and Cope were by preference and temperament theoreticians in a broader sense and tended to consider

morphology as a somewhat wearisome necessity. In personality and in the effect of personality on science, Scott and Osborn happily did not parallel Marsh and Cope. They cooperated with each other, remained firm friends, and passed on a tradition of mutual helpfulness to their successors. They encouraged and aided younger men to enter their profession, and the great expansion of this profession in the last few decades is due in large measure to them.

Without disparagement but as a dispassionate judgment such as he would have welcomed, a review of Scott's career leads to a feeling of regret in just one respect. He tremendously advanced the science of paleontology as he found it, but he made few noteworthy innovations in its methods or principles and contributed little of a fundamental nature for integration with the broader fields of biology or geology. This is the more regrettable and the harder to understand when the achievement and apparent greater promise of his 30's are considered. Already in his 40's, when most biologists or geologists of his stature are reaching greatest fulfillment, his outlook was becoming narrower and his insight into the deeper problems of his subject seems to have ceased to develop, or even to have retrogressed. It is significant that when he heard a discussion of Mendelism and evolution in 1904 his reaction was to repeat "Uncle Jack Robinson's formula, 'Mebbe it is, but I don't believe it.'" It is still more significant that he could write in 1937 that he had found no need "to abandon any of the tentative conclusions concerning the modes and factors of mammalian evolution which were formulated so many years ago." To the end of his life, the only apparent change from his views from the 1890's was an increasing disillusionment. He remained anti-Darwinian and retreated from his cautious neo-Lamarckism without finding anything to replace it, so that evolution became for him simply inexplicable. He had earlier (in 1913) expressed a hope that experimental zoology and paleontology might solve these problems by combining their resources, but he saw no real basis for such a synthesis and did nothing to promote it.

The slight feeling of regret that Scott did not accomplish still more is in itself a tribute to how much he did accomplish. No

one wishes that mediocrity had been more fully expressed, but one does wish that Scott's exceptionally well balanced mind and profound knowledge had been more fully expressed on levels of higher abstraction. Even this appreciative wish would not be a proper note on which to end the evaluation. Scott's disillusionment with attempts to explain evolution and his retreat from theory occurred in a period when the basic study of evolution had fallen into confusion and when many despaired of achieving a reasonable and valid generalization. Some students reacted by proposing solutions that seem, in retrospect, merely bizarre, or by clinging blindly to solutions clearly discredited. Others lost faith in scientific method and sought refuge in views covertly or overtly metaphysical. Scott saw the difficulties and it is wholly to his credit that his intellectual honesty and his faith in objective science prevented his entering the false paths followed by so many of his colleagues.

Scott's unusually long career was also unusually productive. His material contributions to mammalian paleontology fill many volumes. There is hardly any phase of this subject that does not depend today, in some measure, on studies first made by him.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Acad. Nat. Sci. Phila. Proc. = Academy of Natural Sciences, Philadelphia, Proceedings
Amer. Assn. Adv. Sci. Proc. = American Association for the Advancement of Science, Proceedings
Amer. Geol. = American Geologist
Amer. Journ. Sci. = American Journal of Science
Amer. Mus. Nat. Hist. Bull. = American Museum of Natural History, Bulletin
Amer. Nat. = American Naturalist
Amer. Phil. Soc. Proc. = American Philosophical Society, Proceedings
Ann. Mag. Nat. Hist. = Annals and Magazine of Natural History
British Assoc. Adv. Sci. = British Association for the Advancement of Science
Carnegie Mus. Ann. = Carnegie Museum Annals
Contr. E. M. Mus. Geol. Arch. = Contributions from the E. M. Museum of Geology and Archeology of Princeton College
Field Mus. Nat. Hist. Geol. Mem. = Field Museum of Natural History Geology Memoirs
Geol. Mag. = Geological Magazine
Geol. Soc. Amer. Bull. = Geological Society of America Bulletin
Harvard Coll. Mus. Comp. Zool. Bull. = Harvard College, Museum of Comparative Zoology, Bulletin
Internat. Mo. = International Monthly
Journ. Morph. = Journal of Morphology
Journ. Roy. Micro. Soc. = Journal of the Royal Microscopical Society
Morph. Jahr. = Morphologisches Jahrbuch
Princeton Coll. Bull. = Princeton College Bulletin
Princeton Morph. Studies = Princeton Morphological Studies
Quart. Journ. Micro. Sci. = Quarterly Journal of Microscopical Science
Sci. Amer. Suppl. = Scientific American Supplement
Sci. Mo. = Scientific Monthly
Scribner's Mag. = Scribner's Magazine
Smith. Inst. Ann. Rept. = Smithsonian Institution Annual Report
Studies, Morph. Lab. = Studies from the Morphological Laboratory of Cambridge University
Wyo. Hist. Geol. Soc. Proc. Coll. = Wyoming Historical and Geological Society, Proceedings and Collections
Zool. Anz. = Zoologischer Anzeiger

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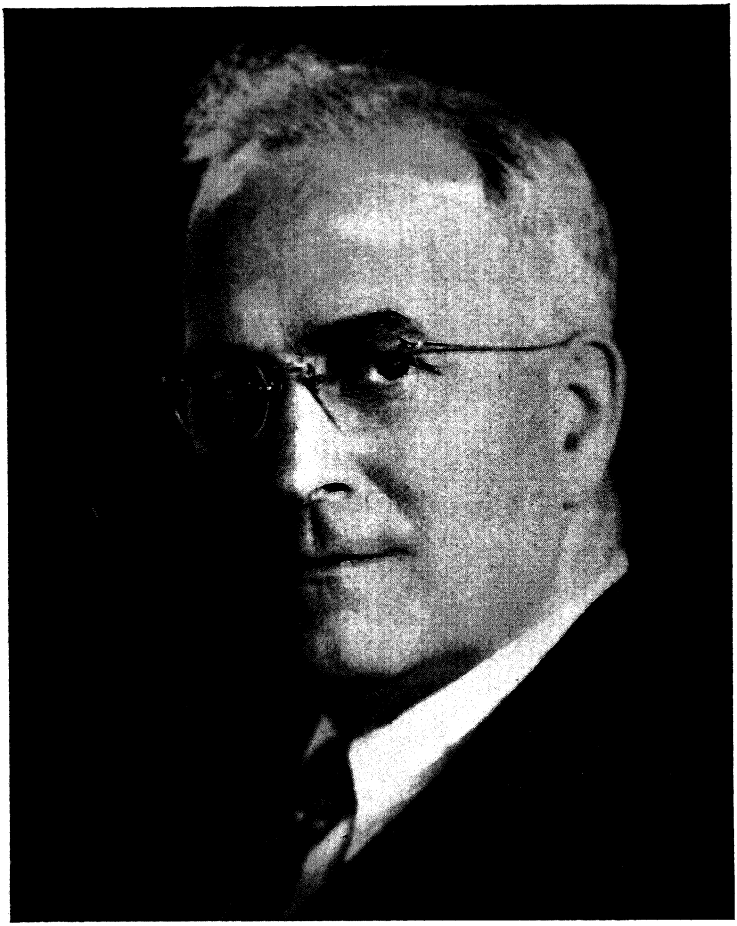

C. V. Taylor

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OF

CHARLES VINCENT TAYLOR

1885–1946

BY

C. H. DANFORTH

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CHARLES VINCENT TAYLOR

1885–1946

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Charles Vincent Taylor, whose death occurred on February 22, 1946, was the descendant of pioneer families who played important and at times spectacular roles in the history of Tennessee. His ancestor, Isaac Taylor, is said to have come to Virginia from County Antrim, Ireland, about 1740 bringing with him several sons, among whom was Andrew I, then a boy about seven years of age. The family settled in Rockbridge, Virginia, where its members later became wealthy landholders and slave owners. Andrew who married first his cousin Elizabeth Wilson, and later her sister Ann, early moved to the now famous Watauga Settlement in the disputed area claimed at that time by both Virginia and North Carolina. Here he was soon made a member of the legislature in the abortive "State of Franklin" and was also a surveyor of more than local renown. He became the progenitor of many distinguished descendants, including General Nathaniel Taylor, Senator Nathaniel Green Taylor (also Commissioner of Indian Affairs under President Jackson), the two Tennessee governors, brothers "Bob and Alf" Taylor, who at one time ran against each other on opposing tickets in Tennessee's "War of the Roses," and of a surprisingly large number of judges, lawyers and other citizens of prominence.

According to penciled notes made by Charles Vincent Taylor, apparently during a visit at the home of Governor Alfred Taylor in "Happy Valley," this branch of the family could also lay claim to an attenuated line of Indian descent which came through Colonel John Carter, father of Langdon Carter, whose name was chosen for Carter County, and for whose wife (Elizabeth Maclin) Elizabethton was named. However, no clear evidence has been found by this biographer that C. V. Taylor himself was a descendant of Langdon Carter, but he may have been, since many of his ancestral lines have not been completely followed out, and there was no doubt a considerable amount of intermarriage in this relatively small community. In the direct Taylor line his

descent seems to have been through Isaac, Andrew I, Andrew II, Jonathan, Andrew D. and Isaac Newton.

At the approach of the Civil War the Taylor family, like many another in the border states, became sharply divided, some joining the Confederacy, others remaining loyal to the Union. Andrew D. Taylor, C. V.'s grandfather, was among the latter, and his three sons, of whom Isaac Newton was the oldest, all joined the Northern Army. While the boys were away in the service, armed men stopped one night at the family home, called the elder Taylor to the door and shot him in cold blood. The sons remained unaware of their father's death for a number of months thereafter.

Isaac Newton Taylor first joined Company B, Fourth Tennessee Volunteer Infantry, but later, apparently with full knowledge of the colonels of both units, he transferred to Company L, First Tennessee Volunteers, in which he served from December 26, 1862 to May 31, 1865. On June 25, following his discharge from the army I. N. Taylor married Christina Bashor, daughter of Henry and Elizabeth (Bowmann) Bashor, well-to-do mill owners of German descent who, like himself, lived near Johnson City, Tennessee. The young couple first lived in a cabin built by the bridegroom, but in 1869 they moved to Missouri and purchased a farm near Whitesville in Andrew County.

From this time on conditions were difficult. Mr. Taylor, now somewhat handicapped physically, was unable to make his farm realize the income it might have yielded, and the family entered upon a period when resources were indeed meager. In 1873, because of disabilities incurred in service, Taylor was granted a pension of $24.00 a month, an appreciable amount for those days. But by 1876 the Pension Bureau had disallowed this pension on the ground that there was no official record of his discharge from the infantry company and, although serving in the cavalry, he was in effect a deserter from the infantry when his disability was incurred. Aside from the heavy financial loss, there seemed to have been a point of honor involved, and over a period of years much effort was devoted to getting the matter clarified. On March 20, 1884, an official certificate of honorable service and discharge was issued.

In religion the early Tennessee Taylors, Wilsons and most others of Scotch-Irish descent apparently were Presbyterians. In later generations some of the Taylors were prominent Methodists and Baptists, but after removing to Missouri this branch of the Taylor family became associated with the Dunkards, or Brethren, to which sect they were zealously devoted. One of I. N. Taylor's daughters later became national director of the dress-reform movement in that church, and the oldest son became a preacher. It is said that before his early death this older brother extracted a promise from Charles V. Taylor, who was then only eleven years old, that he too would preach—a promise which was made good.

These few facts may serve to indicate some of the hereditary background and the environmental setting for Charles Vincent Taylor who, on February 8, 1885, was born into this family, the youngest of ten children, two of whom had died in infancy. His first fourteen years were spent on the Missouri farm, where he early acquired a love for the out-of-doors. Many years later he remarked to one of his colleagues that such an environment quite naturally awakened the interests which were to activate much of his later life.

Another important factor in shaping his early interests was the influence of his first school teacher, Gladys Kent (now Mrs. J. C. Hashor of Savannah, Missouri). Miss Kent seems to have had rare ability to inspire her pupils with ideals of morality and a high quality of intellectual curiosity. Mottoes, in the B. Franklin tradition, and classical quotations pasted on the walls of her school room were remembered by Charles Taylor, and often quoted by him. Many years later he wrote on the back of one of her photographs "Gladys Kent, Rosendale, Missouri. My first teacher. At Crockett country school about ½ mile north of our Missouri home, near Whitesville, Missouri. She, probably more than any other teacher I had, helped me to sense my interest and abilities in gaining a knowledge and understanding of things animate and inanimate in the world around us." Then follows a slightly paraphrased quotation from Shakespeare ("Good name in man or woman, dear my lord, ...") and the notation "Written by G. Kent on fly leaf of a book

'Robinson Crusoe' given me as first prize in spelling." In a letter dated May 19, 1947, Mrs. Hashor herself writes: "Yes, he was one of my first pupils, and I was his first teacher, and as I go back down the years I see an eager, enthusiastic boy, smiling up at me with big brown eyes filled with gratitude for something I had said or done for him. He was a wonderful pupil for four years, with every task faithfully and gladly done, and I always felt he would strive to attain the highest and best in life." She disclaims any credit for herself, extolling the home from which he came and adding that his father and mother "were the kind of parents that give the world sons like Charles Taylor."

At the age of fourteen, he considered himself ready to seek new fields of endeavor, and quite naturally looked toward Mount Morris College, which was under the sponsorship of the Brethren, and to which several of his brothers and sisters had already been attracted. When his father protested that he was too young and the expense would be too great, Charles immediately sold his pony and squirrel gun—property dear to the heart of a boy in his teens—and paid his own transportation to this mecca of his young dreams. From that time on, he was not only self supporting but contributed freely, and often beyond his means, to other members of the family whenever he felt any of them were in need of help. He was especially generous to an invalid sister and to his mother in her later years.

At Mount Morris, he first attended the Academy, taking a business course and living at the near-by home of his sister, Mrs. Nora Wallace. He earned his expenses by acting as "bell-ringer" and doing various odd jobs about the school. A classmate of those days reports that his bell ringing was a model of punctuality except for an occasional retardation in the rising bell. He is remembered from this period for his unfailing good nature, his jovial disposition, and the aptness with which he was wont to characterize his classmates by appropriate nicknames, "but never to anyone's discredit." He was fond of music and had an especially fine bass voice, which put him in great demand for solo and group singing. For a time he considered music as a possible career.

On completion of his business course he secured a position as bookkeeper in a publishing house in Elgin, Illinois, where he worked for some time, but by 1906, with greater maturity and somewhat improved resources, we find him back at Mount Morris College, this time working toward the A. B. degree which he was to receive in 1911. With all his fun and whimsy, he was devoutly religious and according to the custom of the Church of the Brethren he was elected by ballot to the Ministry. This unsought distinction is in itself indicative of the esteem in which he was held by his fellow church members.

During this second period at Mount Morris he was prominent in student affairs and participated in many extra-curricular activities. Among items culled from contemporary numbers of *College Campus,* we find him preaching at a revival meeting, addressing a missionary meeting on "Pure Fun and Worthless Foolishness: Their Effect on Christian Development," winning first honors in a peace contest, a member of the Leaders Club directing an advanced group in physical education, singing in quartets and solo on many occasions, acting as a toastmaster, holding various offices in a literary society, serving as associate editor on the college paper and participating in many other activities. For one or two years he was listed and photographed among the faculty.

Most of his articles in those days involved a blend of philosophical and scientific thinking, with a strongly religious flavor. In "The Greatest Conqueror" (Jesus) he alludes to the conquests of Alexander, Hannibal, Caesar, Grant and others, all of which pale by comparison with the conquest at Gethsemane. In an article "Man a Metaphysical Animal" (listed in the table of contents, no doubt to his amused annoyance, as "Man a Physical Animal"), he compares the development of individual human interests and scientific yearnings with those of the human race as a whole. This was in the manner of the evolutionist, suggesting a kind of recapitulation, but it is obvious that it contained nothing in fact or attitude which he regarded as in any way inconsistent with orthodox religion. The article closes with ". . . and so we shall ever muse and wonder, because we

have come into existence 'not in entire forgetfulness . . . from God who is our home'."

During this second period at Mount Morris, his interests in biology were intensifying, and in view of his later major preoccupation, it is of interest to find a frontispiece in the annual catalogue of Mount Morris College for 1906-1907 showing the detailed anatomy of a "*Paramoecium* drawn from life by Charles Taylor of the biology class." His determination to follow biology as a profession seems to have begun to take shape at about this time. At about this time, too, he probably began to be assailed by the doubts and mental conflicts which are common to most students of biology who have earlier had orthodox religious training. But whatever these doubts may have been, he apparently solved his own problems by himself and emerged an honest agnostic with no pretense at finality of judgment in religious matters. Of course there was no longer any question of his continuing in the ministry, but the training and orientation of his earlier days left an enduring impression on his character.

Following graduation from Mount Morris he became principal of the high school in Valley City, North Dakota. At that time the school had a faculty of ten or twelve members, and he remained in charge from September, 1911, to June, 1914, when he left for California to do further work in zoology. One of the probable reasons for this temporary departure from the career which he had by now determined upon was no doubt the desire to obtain more adequate funds, both to help a sister who was ill at the time and the better to provide for his own graduate study.

At the University of California, Taylor entered a large department, with diversified interests, where he was able rapidly to broaden his horizons and gain further insight into the scope of his chosen field. In this new environment his abilities and aptitudes were soon perceived and utilized. The University records show that he was assistant in zoology from 1915 to 1917; teaching fellow, 1917-1918; instructor in protozoology, 1918-1919; assistant professor, 1920-1925. The same fine qualities of sincerity, geniality and idealism which had distinguished him at Mount Morris were equally evident at the University of

California where he made friends among both faculty and students. His colleagues of those days speak warmly of his personality and of his abilities as a teacher.

The first serious research problem which he undertook at the University of California was done under the direction of Professor J. A. Long and dealt with behavior of the sperm head and organization of pronuclei following fertilization in the mouse. Although both teacher and pupil have long been known as masters in the development and application of clever manipulative procedures, in this particular study only conventional methods were employed. It is interesting, too, that this is almost the only occasion when Taylor ever ventured to do research in the mammalian field, the hope then expressed of extending his studies to cleavage stages in the mouse ovum never having been realized. Under the title "Some fertilization stages in the mouse" a report on this work was accepted in partial satisfaction of the requirements for the degree of Master of Arts, which was awarded to him on May 15, 1916.

The award of his Ph.D. degree came on December 20, 1918. His doctoral dissertation, done under the supervision of Professor Charles A. Kofoid, was entitled "Demonstration of the function of the neuromotor apparatus in Euplotes by the method of microdissection." This dissertation was published October 23, 1920 in the *University of California Publications in Zoology.* A previous note on "The neuromotor system of Euplotes" had preceded the main paper by about a year. On purely theoretical grounds Kofoid had earlier designated certain fibrils detectible in the protoplasm of the protozoa as a neuromuscular apparatus, and Yocum had presented morphological evidence in support of this view, but Taylor felt that however convincing that evidence might seem to be, methods beyond the limits of ordinary morphological techniques would be necessary to prove, or disprove, the neuromuscular nature of the fibrils. The Barber micropipette, with recent modifications and improvements by Chambers (a close friend in later years), provided the desired tool with which to begin the investigation, and also served as an inspiration for the elaboration of Taylor's own ingenious and remarkably effective microdissection apparatus.

In these early papers a detailed description of the structure and behavior of *Euplotes patella* is followed by a clear-cut and convincing experimental analysis of the fibrillar apparatus, which proved to be neither contractile nor supporting, but strictly a conducting system. At this time he emphasizes, as he continued to do for years to come, that the protozoa are not merely simple cells, but highly complex and coordinated organisms. His doctoral dissertation gave clear indication of Dr. Taylor's chief interests and aptitudes and pointed the direction which his steady series of publications would take during the next quarter of a century. By ingenious and original methods he added greatly to the knowledge of the organelles of various protozoa, especially *Euplotes,* one species of which (*E. taylori*) appropriately bears his name. Another study of considerable importance at that time was the demonstration of the role of the micronucleus in *Euplotes,* where it was shown that if this organelle is removed the protozoan ceases to undergo division, but if it is replaced reproduction continues as before. Among later publications a great deal of attention was given to problems of encystment and excystment. In the latter studies a new species, *Colpoda duodenaria,* described by himself and Dr. Waldo Furgason, proved especially valuable.

For further indication of his earlier scientific contributions, as well as his later ones, reference may be made to the readily available papers listed at the end of this article. Dr. Taylor's own summary, characteristically brief, and written not long before he died is as follows: "Research contributions (a) on living cells: function of fibrillar systems; role of micronucleus; development of egg fragments; polarity in normal and centrifuged ova; x-ray effects; lethal effects of x-rayed media; high vacua and extreme temperature effects; induced encystment and excystment; cell growth factors; and (b) on protoplasm: sol-gel reversibility; cataphoresis of ultramicroscopic inclusions; bio-electric potentials; cytoplasmic reorganization; nuclear reorganization; reversible protoplasmic structure. . . . Of the various contributions made to science, probably the most important would be (a) experimental evidence of protoplasmic reorganization during various cell cycles, (b) tying this in

with the sol-gel reversibility and other physical properties of living cells."

So far as can be judged at this time, Dr. Taylor's estimate of his own work was accurate and well-balanced, although the significance of what he did, and its influence on fellow protozoologists, was undoubtedly greater than he himself implied, or perhaps even realized. His investigations on the organelles of protozoa, especially the pioneer determination of the true nature of the fibrillar apparatus in *Euplotes,* not only established important new facts but set an admirable pattern of research in the field. His demonstration of the function of the micronucleus in the same organism was of great significance in both protozoology and genetics, and, since it was so, served to emphasize still more the value of the microdissection method. Another phase of his work which deserves additional emphasis is the analysis of the effects of x-rays on protozoa. He showed that alterations due to radiation may be observed not only in protozoa which have themselves been exposed, but even in unexposed specimens placed in previously irradiated media. The importance of the latter finding is probably better appreciated now than when the observation was first made, but Taylor's studies in the field undoubtedly have from the first had a significant influence on the thinking of protozoologists.

Perhaps the outstanding characteristic of his scientific work was its consistently high quality and the technical skill with which his experiments were conducted. His endeavor to reduce all experimental situations to their simplest form is well exemplified by his persistent effort, over a period of years, to attain a purely synthetic medium which would permit him to grow experimental forms for the study of encystment and excystment under completely controlled conditions. In the laboratory his meticulous care and scientific skill impressed students and collaborators almost as much as the results to which they led. His influence was thus extended by his example no less than by his findings.

During the eleven years in which he was connected with the University of California Dr. Taylor had opportunity to broaden his contacts further through temporary appointments at other

institutions. He visited the Marine Biological Laboratory at Woods Hole where he met and collaborated with Dr. Chambers; was Johnston Scholar at Johns Hopkins University, 1918-1920; acting assistant professor at the Hopkins Marine Station of Stanford University in the summers of 1922 and 1923; assistant professor at the University of Michigan in 1923-1924; and Research Associate at the Tortugas Laboratory of the Carnegie Institution in 1924 (and again in 1926).

An event of importance in the University of California period was his marriage on May 6, 1921 to Lola Lucile Felder. Miss Felder, who had been a student in one of his classes, brought to their marriage a spontaneity of spirit and an artistic outlook which to no small degree mirrored latent or suppressed components in Charles Taylor's own makeup, but she was never able to free him entirely from intolerance of the "worthless foolishness" which he had long ago deprecated. From the time of his marriage, he was "CV" at home and to most of his friends and associates.

His first summer at the Hopkins Marine Station seems to have been a particularly stimulating one, and he entertained high hopes of being able to enlist the cooperation of the University of California in making this a fully equipped marine station dedicated to the training of advanced students in biology at the two sister institutions. Such a plan seems to have won the approval of the director, Professor W. K. Fisher, but at the University of California CV met with the academic inertia which he often found so trying to the spirit. He returned to the Station in 1923 with one or two graduate students but he had by then temporarily given up hope of seeing the laboratory the joint venture of which he had dreamed. The next summer he went to the Tortugas Laboratory, renting a house in Sebring, Florida, for his wife and children, his mother and two of his sisters. However, his connection with the Hopkins Laboratory was by no means terminated, for when he joined the Stanford Faculty in 1925 he was made associate director, a position which he held for the rest of his life.

Dr. Taylor's appointment at Stanford was due in no small measure to the favorable impression he had made at the Hopkins

Marine Station, and the director of that Station was active in urging his subsequent appointments and promotions; but Taylor quickly made his abilities felt and needed no special backers. By the time he moved to Stanford he had developed a mature outlook and a definitive orientation toward life and his profession. He impressed his associates above all by his idealism and his devotion to science on its highest planes. He emanated a spirit of scientific fervor, almost religious in character, which was inspiring to students and colleagues alike. In him, one saw a man who genuinely believed in the preeminence of science, who would do his best to promote it, and who would be on the alert to defend it against subversive influences of all sorts. That he was doomed to disappointments and a measure of bitterness later was not apparent in those days. While it might have been better for his peace of mind if he had not fallen heir to administrative duties and executive responsibilities, nevertheless, from the very first he seemed especially fitted for such assignments.

Dean Taylor's relation to the School of Biological Sciences at Stanford can best be understood in terms of the history of the school and his own philosophy of science. In the early days at Stanford, individual departments enjoyed an unusual degree of independence and autonomy in budgetary matters and in the control of both graduate and undergraduate students. But for some years before Dr. Taylor arrived efforts had been made to weaken the barriers between what were facetiously called "the water-tight compartments" of the University. Previously a few courses, especially those given by President Jordan, had treated biology from a broad point of view, but not till 1919-1920 did "Biology" appear in the annual Register of the University as a distinct entity. In that year a general course, somewhat of the "survey" type, was presented by President Wilbur and members of the departments of Botany, Entomology, Physiology and Zoology. This was the germ from which the School of Biological Sciences was to develop. The first year the course was listed as if it were a division in the Department of Botany, and it is interesting that later on Botany was the first Department to be absorbed by it. The following year lecturers were brought in from Paleontology and Psychology, and the man in

charge of the laboratories was listed as acting assistant professor of *biology*, possibly the first teacher at Stanford to have "biology" appear in his title.

During succeeding years "Biology" made steady progress, soon acquiring a status coordinate with Botany, Zoology, and other major departments; and then, in a sense, it absorbed them. By 1925-1926, when Dr. Taylor's name first appeared, several departments had already been ingested and "Biological Sciences," listed in the Register as if coordinate with most major departments in the University, now included as divisions, the School of Biology with an executive committee of ten members, General Biology with several courses and an administrative committee, Botany, Hopkins Marine Station, Physiology and Zoology. The following year Dr. Taylor became a member of the executive committee and Public Health Nursing, Anatomy, and Bacteriology and Experimental Pathology, were first definitely included in the grouping under Biological Sciences. In 1929 the name of the new aggregate was officially changed to School of Biological Sciences and its status somewhat, but by no means completely, clarified.

At this point Dr. Taylor's connection with the School was interrupted for a brief period. In 1930 he received an especially attractive offer from the University of Michigan, which he considered seriously for a time, but his wife's health and what he considered as favorable prospects for the School of Biological Sciences at Stanford decided him against acceptance. When this decision was finally made, he conscientiously returned the money which Michigan had advanced to cover the cost of a visit to Ann Arbor and paid for the trip out of his own pocket. He did, however, take a leave of absence for the year 1930-1931, during which he acted as visiting professor of zoology at the University of Chicago.

On his return to Stanford he was made Herzstein Professor of Biology and, in 1933, Chairman of the School of Biological Sciences. This position was equivalent to that of dean, but he was not officially given that designation until several years later. At the time he became chairman, or dean, ten departments were listed as falling wholly or in part within the School and, in

addition General Biology, offering five courses by various instructors, was also included as still another entity.

It will be apparent from the preceding paragraphs that the School which Dr. Taylor was asked to head was one which had evolved in part from a single course and in part from the absorption of pre-existing departments, and that technically it was still ill-defined as to content and scope. Developments at Stanford, of course, reflected, in their own way, a movement that was widespread at the time, but it is not surprising that some of the older men looked upon the new school as an aggressive parasite that was sapping the life blood of the traditionally established departments. Dr. Taylor himself, if he could have viewed developments from a little greater distance, would no doubt have detected some interesting parallelism between the evolution of the school and the ontogeny of a living organism. While in no way responsible for its initiation and early development, he was sympathetic with the trend and anxious to further its advancement along sound lines of teaching and research.

The problems with which he was confronted as dean of the school included the disposition of such, now "vestigial," departments as Botany and Zoology and the coordination of work in departments which, because of their diverse affiliations, were not assimilable. The latter difficulty remained partially unresolved until some of the medical departments were finally removed from the School in their listing and in their administration. A long step toward solution of the other main problem was taken during Dr. Taylor's first year as chairman when, presumably at his suggestion, the trustees officially abolished the departments of Botany and Zoology, assigning all members of these departments to comparable ranks in Biology. It is due in no small degree to confidence in Dean Taylor's sincerity and his devotion to the best interests of science that this final step in the dissolution of formerly strong departments was accomplished with a minimum of opposition or dissatisfaction.

The years following the primary reorganization showed frequent further shifts and rearrangements within the School, but the general drift is intelligible in terms of Dr. Taylor's outlook

on the field of biology as a whole. It might be, and indeed has been, argued that it makes little difference whether a degree be granted in "Botany," which is admittedly a biological subject or in "Biology (Botany)," as was prescribed a short time after Dr. Taylor became dean. But to Dean Taylor there was a fundamental difference. The central tenet of his philosophy was the unity of all life, and to him the various biological disciplines merely represented different aspects of a single whole. Where some students like to emphasize the divergencies in biology, he was impressed by factors that are common to all the manifestations of life. Recognition of a basic unifying principle was to him a *sine qua non* of sound biological scholarship and teaching. He extended this line of thought to fields outside biology and was convinced that there is little hope of genuine advancement unless students in the social sciences, humanities and politics can achieve an essentially biological approach. He believed thoroughly in always having biology presented from this unitarian point of view; and so to him it did make a real difference whether the degree was in *Botany* or *Biology (Botany)*. The connotation of the two expressions are obviously different. One emphasizes that botany is a facet of biology, the other does not.

In meetings of the executive committee and of the faculty he labored this point and its ramifications with great earnestness. Few were the meetings when he did not turn to the blackboard and emphasize his points with diagrams such as circles within a circle, a tree with concentric rings and branches, each representing an aspect or division of the subject, until one almost gained the impression that these figures had come to mean more to him than mere symbols. One diagram in particular, a cube done in colors and shown as if sectioned in all three planes to reveal interrelations of morphological, physiological and developmental aspects of various categories, was displayed in his office for a number of years, and not infrequently brought to faculty meetings.

His conviction as to what is fundamental and significant in education tended to make him apprehensive of the real or fancied desire of the professional schools to exercise too great a control

over undergraduate curricula. He felt that professional training should be based on a sound foundation in biology, and to this end fought long and hard to keep botany, for example, a requirement for all degrees in the school, whether pre-professional or not. In time, with the loyal and sympathetic cooperation of his executive committee, the curriculum of the school was whipped into a well-knit unit which gave a tangible expression to the ideals which he cherished. But he continued to feel the danger of encroachment from without and particularly feared that his own university, along with other private institutions, might come under the influence of agencies which might debase the ideals of true scholarship. He felt, too, that the authorities did not adequately support him in his endeavors to combat these tendencies. Finally it was with special bitterness that he came to realize that his own faculty could not stand solidly behind him in his opposition to certain developments on the campus or in his methods of combatting them. This and his slowly developing fatal illness (leukemia) made his final months a period of discouragement and sadness.

Time-consuming, and to a degree unrewarding, administrative duties were necessary concomitants of his position, and he devoted himself to them without stint. In general tolerant and humorous, there were rare occasions when his patience gave way completely and he became arbitrary and dictatorial, but such times were indeed few. His pleasanter and more valuable functions as teacher and promoter of research were the happier ones. With extraordinary ability to recognize superior talent and promise, he gathered around him one of the most outstanding groups of biologists in the country. He was excessively loyal to his staff and was active and effective in raising money in support of their research and in otherwise furthering their interests. One of them writes "Although stubborn in his adherence to a few cherished convictions and objectives, he gave unquestioning support and freedom to his staff within their individual spheres of responsibility. . . . His talents and horizons as an organizer were well exemplified by the highly successful Cell Symposium held at Stanford in 1939 [in celebration of the 100th anniversary of the cell theory] to which he attracted

a distinguished array of internationally known scientists." The papers presented at this symposium were collected by Dr. Taylor and published in "The Cell and Protoplasm" (1940).

He particularly loved a quiet evening at home with the four children Jeanne, Elouise, Lenore and Isaac Newton of whom he was very proud, or with one or more intimates when "our conversation always came around to the unity of science." But in spite of innate social tendencies he had a kind of personal "reserve that was almost Indian in quality," and his biographer repeatedly was confronted with such statements as "Now that I think of it, I never really *knew* CV." However, in a letter written in 1934 during a trip to Europe when he visited German relatives and attended the International Zoological Congress at Venice, he confided to Professor Fisher: "I have never before known what vacation is like, and how much it can benefit one in body and soul. The world is one thing off there in a little corner in California, but quite another thing in reality and not in books," and then went on to suggest the desirability of a rule that everyone who professes to know the living world must travel, "the farther and more the better." He did not follow his own recommendation to any great extent, but he did find much pleasure in organizing camping trips into the Sierra for visiting biological friends.

A colleague from one of the other schools of the University writes: "Taylor was a man of unusual vigilance and insight into the significance of what was going on about him in the university world, and he was quick to see through shams. He was keenly aware of the true nature and requirements of a great university and of what constitutes a real scholar. He was vigorous in upholding academic standards and opposed the many tendencies to lower them."

He was a member of, and took an active interest in, the National Academy of Sciences (elected in 1943), the Society of Sigma Xi, the American Association for the Advancement of Science, the American Society of Zoologists, the American Society of Naturalists, the Pacific Oceanographic Society, the Society for Experimental Biology and Medicine and the Western Society of Naturalists. It was largely through his efforts

that the latter society (which he served as president and in other capacities) was revivified following a period of decline in the thirties. He served on the editorial board of *Physiological Zoology* from its inception in 1928. A survey of his scientific papers reveals a close application throughout his productive period to a few basic problems which he investigated with clear insight and great technical skill. Since he believed strongly in the universality of biological truth, he saw no reason to seek widely for research materials and because of this concentration he was able to make significant additions to biological knowledge at a basic level. More than almost anyone else he brought home the fact that the individual protozoan cell, possessing remarkable capacity for reversibility, can pass through phases of development and differentiation which most zoologists have associated only with metazoan forms.

Dr. Taylor's published contributions to biology are listed herewith and are permanently available to anyone who may wish to review developments in this field. It has seemed equally important to emphasize in his biography the circumstances under which he worked and the ideals and attitudes, which have left a lasting impression on his many students and associates. Here, too, C. V. Taylor made a real contribution to the advancement of science.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Am. Nat. = American Naturalist
Ann. Rev. Physiol. = Annual Review of Physiology
Arch. Protistenk. = Archiv fur Protistenkunde
Biol. Rev. = Biological Reviews
Coll. Net = Collecting Net
Jour. Cell. Comp. Physiol. = Journal of Cellular and Comparative Physiology
Jour. Exp. Zool. = Journal of Experimental Zoology
Jour. Gen. Physiol. = Journal of General Physiology
Jour. Morph. = Journal of Morphology
Physiol. Zool. = Physiological Zoology
Proc. Soc. Exp. Biol. and Med. = Proceedings, Society for Experimental Biology and Medicine
Univ. Calif. Publ. Zool. = University of California Publications in Zoology

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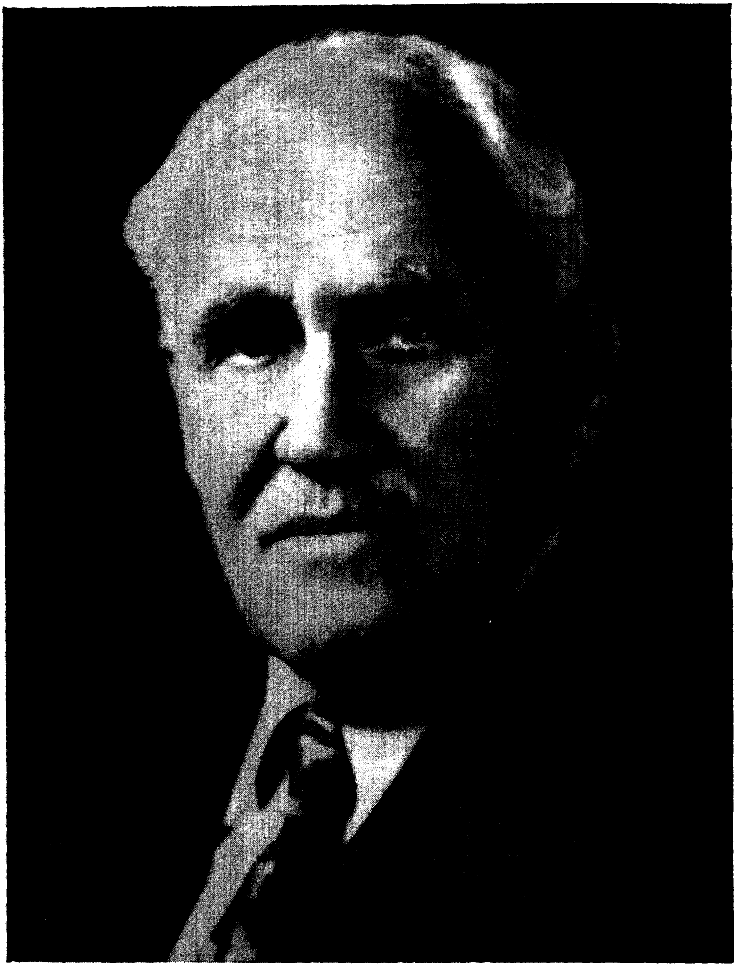

R. A. Harper

NATIONAL ACADEMY OF SCIENCES
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BIOGRAPHICAL MEMOIRS
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BIOGRAPHICAL MEMOIR

OF

ROBERT ALMER HARPER

1862–1946

BY

CHARLES THOM

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1948

ROBERT A. HARPER—CHRONOLOGY

1862—Born at LeClaire, Iowa, Jan. 21
1863—Parents moved to Port Byron, Ill. Elementary education at Port Byron
1882—Entered Oberlin College
1886—A.B. Oberlin College
1886–88—Taught Greek and Latin in Gates College, Neligh, Nebraska
1888 (Fall)—Graduate Student, Johns Hopkins University
1889 (Spring)—Taught science and mathematics at State Normal School, California, Pa.
1889–91—Master in Sciences, Lake Forest Academy
1891—A.M. Oberlin College
1891–96—Professor of Botany and Geology, Lake Forest College
1894–96—On leave for study in Europe
94–95—At Bonn, with Strasburger
95 (Spring)—At Munster with Brefeld
95–96—At Bonn
1896–98—Professor of Biology, Lake Forest College
1898—Professor and Head, of Botany, University of Wisconsin
1899—Married Alice Jean McQueen, who died 1909
1909—Elected: American Philosophical Society
1910—Elected: Phi Beta Kappa—Oberlin
1910—Sigma Xi—University of Wisconsin
1911—Member National Academy of Sciences
1911—February to August—Visiting Professor University of California
1911–1930—Torrey Professor and Head of Department of Botany, Columbia University
1911—Member of Torrey Botanical Club, President 1914, 15, 16
1911–1942—Member of Board of Managers of N. Y. Botanical Garden
1916—President Botanical Society of America
1918–1933—Chairman of Scientific Directors, N. Y. Botanical Garden
1918—Married Helen Sherman
1923–24—Chairman of Division of Biology and Agriculture, National Research Council
1930—Professor Emeritus
1938—Retired to his farm near Bedford, Virginia
1945—Donated his reprint collection of 15000 units to New York Botanical Garden
1946—Died May 12.

ROBERT ALMER HARPER

1862–1946

BY CHARLES THOM

Robert Almer Harper[1] was born at LeClaire, Iowa, on January 21, 1862. His parents moved to Port Byron the next year. His father, Almer Sexton Harper, was a Congregational Minister, a graduate of Oberlin College, and of the Oberlin Theological Seminary in 1853. His wife Eunice Thompson from New York and New Jersey antecedents, had been a classmate at Oberlin. Both were actively connected with educational projects as well as church work. Their three sons, Edward Thompson, Robert Almer and Eugene Howard were thus brought up in an atmosphere of scholarship. Edward after a boyhood interest in plants turned to theology, took his doctorate in Leipzig and became a professor in Chicago Theological Seminary. Robert stayed with botany and Eugene went to zoology but eventually turned to farming.

Dodge in his memoir followed the Harpers one generation further back. Almer Sexton Harper was born in Indiana in 1826, the ninth child, his father Edward Harper was born in 1779 in the Charleston district of South Carolina. Edward Harper married Charity Reed, a school teacher from Connecticut. After several moves in the Carolinas, mostly in pioneer villages, we find them with five children moving to

[1] Professor Harper collected no biographical data. He evidently put little value upon such material in spite of much study in the field of plant genetics. The writer of this memoir was the first graduate student to take a degree with him (A.M. Lake Forest 1897). Personal relations as a student had begun in 1889 and casual contact continued until retirement. He acknowledges freely using the various published notices and memoirs of Dr. Harper, especially those of Stout (Journal of N. Y. Botanical Garden 47 (563): 267-269, 1946); Dodge (American Philosophical Society Yearbook 1946: 304-313); the committee report (MS) of Dodge, Karling, Trelease, and Matzke to the graduate Faculty of Pure Science of Columbia University. The chronology of the years at Lake Forest was furnished by the alumni secretary, Mrs. E. C. Fleming. The bibliography was prepared from the files of the Department of Botany of Columbia University by Miss Sally MacDonald, Departmental Secretary. Dr. B. O. Dodge, Dr. A. B. Stout, Dr. L. O. Kunkel, Professors S. F. Trelease, E. B. Matzke and C. E. Allen have contributed from their personal memories and Mrs. Helen S. Harper has checked data carefully.

Indiana about 1815. The four younger children, among them Almer, were born in Indiana.

Professor Harper thus represented one of the families which followed closely the advancing front of settlement of the central west and carried with them the best ideals of religion and education from the Atlantic seaboard states. These ideals and mental aspects were thus a composite from a varied ancestry which included components from the spiritual heritage of the Carolinas, from Connecticut, from New York and New Jersey, plus the welding power of pioneer experience which broke many and built others to commanding stature.

Robert followed his parents and his older brother Edward to Oberlin, where he took his bachelor's degree in 1886. In spite of his scientific inclinations, we find him teaching Latin and Greek for the next two years, in Gates College, Neligh, Nebraska. He went back to botany, however, in the fall of 1888 as a graduate student at Johns Hopkins University. He stayed there only a few months, since we find him teaching a list of sciences in a Pennsylvania teachers' college during the latter part of the academic year.

In the fall of 1889, he became Master in Science at Lake Forest Academy, Lake Forest, Illinois, where he served during two academic years. The high quality of his scholarship and teaching ability combined with an impressive personality as shown in those two years, led Lake Forest University to shift him to Lake Forest College as Professor of Botany in the fall of 1891. He had received his A.M. from Oberlin in the spring. For a time the title was Professor of Botany and Geology but since few courses in geology were called for, geology was dropped from the title. The coming of the great botanist John M. Coulter to Lake Forest as President in the spring of 1893, offered a favorable opportunity for Harper to take leave of absence for two years for graduate study in Germany (1894-5 and 1895-6).

Harper was already keenly interested in the cell (cytology) and secondly in fungi. Those who worked with him at that time were never allowed to lose sight of the cell as the primary unit of structure. Strasburger's Zellbilding and Zelltheilung

was his "standby." Naturally he went directly to Bonn where Strasburger was a great figure among German cytologists. During part of this two year period he went on to Brefeld's laboratory, an outstanding center of fungous investigations. Exactly how he divided the time is not recorded. His praise of Brefeld's contribtuions as a pioneer in fungous morphology as determined by culture, was always tempered by recognition of Brefeld's [2] stubborn adherence to methods that were already outmoded and conclusions of his own which were already questioned. Eventually Harper found more congenial territory back at Bonn, where Fairchild, Swingle, Osterhout and Mottier were fellow workers.

His research thinking had already followed especially three lines, (1) the structure of the nucleus and its relation to sex especially in fungi; (2) the multinucleate cell as seen in the ascus, in the sporangium of the mucors, in coenocytic organisms and in the naked protoplasmic plasmodium of the myxomycetes; (3) the cell in its transformation as the structural unit of every complex organism that he studied, plant or animal, this covering the field later known as morphogenesis.

In the Bonn papers, Harper considered the first two categories. His students had long heard him discuss the fungous nucleus and its relation to sex. He often discussed the so-called "free cell formation" resident in the puzzle of how eight nuclei could each cut out its unit of cytoplasm so that eight apparently equal, uniformly marked spores would lie with possible unused cytoplasm in the ripe ascus. His clearcut figures depicted the nucleus definitely as the active center around which a unit of cytoplasm was cut off by strands emanating from a central body at the pole of the nucleus. The work was done upon Sphaerotheca but appears to be a fundamental contribution to our knowledge of the development of the fungous ascospore.

The series made up an outstanding contribution not only to fungous cytology but to workmanship. Any one who has followed Harper's method in finishing a cell drawing in india ink, under a handlens, realizes his skill, his patience, and the

[2] The writer encountered Brefeld later in Berlin. Knowing Harper, he was not surprised that he did not stay long with Brefeld.

exacting nature of the observations required. That last paper (1897) was a model which so impressed itself upon the work of students in his laboratories that any one reading a subsequent thesis automatically recognized the Harper influence.

The problem of the ascospore led directly to the sporangium of the mucors. The multinucleate sporangium of a mucor breaks up its mass completely, somehow to form a multitude of spores. Similarly a myxomycete plasmodium with thousands of nuclei but with no cell wall at all in the whole vegetative phase, suddenly turns into a mass of spores with characteristic walls. The ascus had been covered in Strasburger's laboratory at Bonn. Those of us who knew him the next year at Lake Forest saw him attack Pilobolus. During the next ten years, he went back several times to the myxomycetes to puzzle over their spore producing process.

Each myxomycete plasmodium contains countless numbers of nuclei yet without sign of cell wall or apparent relation to particular masses of cytoplasm. Then when fruiting time comes, each nucleus cuts out for itself a unit of characteristic size, which surrounds itself with a wall with the markings of its species, and when set free participates in reproducing the whole cycle. How to reconcile such a procedure with the ordinary cellular process was his puzzle. Nevertheless the myxomycete ends its life story with a definitely cellular unit as a propagating body. Thus there was a common bond in all these protoplasmic masses in that as they reached the fruiting stage they became definitely cellular. But the multinucleate condition shows up in many other groups and there again it always troubled him.

He watched Debski study nuclear division in Chara during his last year at Bonn. Apparently he was not satisfied for he made me repeat the work at Lake Forest during the next year. He summed up his contact with the coenocytes in two papers with a five year interval between them. After so many years of study of the nucleus and cellular organization he just naturally turned to morphogenesis and found in the finished morphology of Gonium, Pediastrum, Volvox, Hydrodictyon and in the Acrasieae (Dictyostelium and Polysphondylium)

striking genera, in which cells apparently equal and capable of independent life could respond to the tensions of organized life by assuming bizarre structures contributory to the final organized unit. Inheritance, then, in such cells included ability to respond, by filling, in more or less definitely predictable degree, the tasks thrown upon the cell by the incidents of organization. The forces which seized upon the single cell as a building block, and caused it to take its place in such weird figures as Hydrodictyon, Pediastrum, Polysphondylium, and Dictyostelium, were troubling him clear back to the days when I worked with him.

Looking over Harper's bibliography, these same three groups of problems stand out. No one could live with him for a few years, then meet him now and then over forty more, as I did, without knowing that he read voraciously and critically in many fields. He used all kinds of material in class-room lectures but published no papers from his reading. Outdoors there were few growing things he did not recognize, and he was not unacquainted with those that crawled. But he did not write about many of them.

Harper defied the dictum that a man is appreciated for the weight of his publications (on the "hay" scales); he published only when he felt that he had something of importance to contribute. Yet he was automatically recognized as among the great botanists of his time.

His career covered the whole rise of plant pathology as a professional field: he joined the society. Dodge says he was a practical plant pathologist on his own farm and in the council of the Botanical Garden, but he did not write a single paper about a plant disease as such.

He saw the rise of genetics—no one could work intensively for ten years upon the nucleus and ten more upon morphogenesis without coming in contact with the whole ground work of modern genetics. Characteristically skeptical of the idea that living things would faithfully follow mathematical formulas, he seized upon factors in corn which seemed to blend in the hybrid—rather than be represented by plus or minus signs, and put several seasons into throwing doubt upon the concept of im-

mutable hypothetical units of inheritance concocted to account for selected results.

Harper was not satisfied with the iron clad concept of nuclear organization which hypothesizes such fixedness of its mechanism as tied it to inevitable fate. To him it was a thing alive and there is a fluidity about living; it was able to adjust itself to changing demands, at least, to a difficultly predictable degree. If it carried factors determining the morphology of its species in its chromosomes, these chromatic elements were so related to other components of actively circulating protoplasm as to put their impress upon the whole cell without realistic fatalism. To him, the concept of inheritance illustrated by a lot of beads strung upon a wire, might suggest as many misconceptions as truths. Fully alive to the value of hypotheses, he still took much pleasure in puncturing "balloons" that he believed to have doubtful continuing value. Such an attitude might appear destructive but it must be remembered that to one who has worked widely in the biological field and with many types of investigation the mechanical concept of inheritance leaves the mind often unsatisfied.

He was elected to the National Academy of Sciences in 1911. He belonged to a number of professional societies in which a man becomes a member because he wants to work with others in either the narrow field of one discipline or in a broader aggregate of many types of training. Among the botanical groups we find: The Botanical Society of America (President 1916), The Torrey Botanical Club (President 1914-15-16), Linnean Society of London, Corresponding member of Deutschen Botanischen Gesellschaft, Phytopathological Society, Ecological Society.

In the broader field: The American Association for the Advancement of Science (V. President. Sec. G, 1910), American Academy of Arts and Sciences, American Philosophical Society, The Wisconsin Academy of Sciences, Arts and Letters, New York Academy of Sciences, Washington Academy of Sciences, the Century Association of New York City.

In his own biographical notices in Who's Who in America and American Men of Science, he omitted mentioning Honorary

Doctorates of Science from Columbia University and the University of Pennsylvania. These must be included in this record.

Certain types of responsibility naturally went with his professorship. He was Chairman of the Board of Scientific Directors of the New York Botanical Garden from 1918-1933; he was a member of the Board of Managers from 1911 to 1942. He kept an office there and spent at least one day of the week there for many years. Naturally his collection of reprints (15000) was given to the Garden before his death. He served also as a Director of the Tropical Plant Research Foundation, and of the Boyce Thompson Institute. Those who worked with him in these relationships bear testimony to his active participation in working out the problems encountered.

He taught botany for forty years. A lot of us at one time or another listened to his lectures, and argued with him in his study or at his microscope table. He contributed unstintingly of himself. The impress of his ideals of scholarship has reached several generations of students. What manner of man was he?

Harper called himself a botanist; he included all plant study under the name. His published papers are not particularly numerous, but practically all of them covered problems fundamental to plant life. Though he was interested in practical as well as theoretical phases of the subject, he never called himself anything but a botanist. He was a collector with experience and skill—he knew where to go to get what he wanted. He took his classes to the fields, to the dunes, to the hills; he was no stranger in the swamp or on the waterside—he had just one name for it, botany. Back of that, he knew a lot about animal life. He was thoroughly saturated with biology—the idea that all living things had a mass of fundamentals in common. One had to be a botanist because there was not time enough for everything.

He was not a spell-binder; he would never have been popular upon the Chautauqua circuit. In his early class-room lectures, he put biological details together so compactly as to tax the best student's ability to take notes. Professor Matzke

says that, "In later years, his lectures were beautifully prepared, forcefully delivered, filled with provocative thoughts and suggestions for further research." That same background of accurate scholarship made him a keen critic of publications which expanded a minor idea to great length and never used a short word if a long one could be found. Our seminar in the winter of 1896-7, consisted of Harper, Timberlake and Thom—he passed to Timberlake a new book by a distinguished professor (afterward a colleague of his), to read a chapter. Then as a long paragraph was finished and another was started, Harper spoke—"Stop! Skip that next paragraph—that's just a mess of long words." A discussion must be adequate and accurate but not merely prolonged.

The student without a rigorous background of English composition faced an ordeal when he started to write a thesis for Harper. He had little tolerance for verbosity and less for loose thinking. He was a direct man. He wanted to know exactly what you meant in every statement—he expected you to have mastered the doctrine as he laid it down, to dispute it, if you could make your point stick. No one regretted his rigorous demands after the task was completed.

Something of his mental attitude may be seen in the advice he gave the writer when he faced a scientific meeting with his first paper. "If you have an idea that you wish your audience to carry away, turn it upside down and inside out, rephrasing it from different angles. Remember that the form in which the thing may appear best to you may not impress half your audience." He added that a miscellaneous audience can not be expected to carry away a lot of separate facts but one good idea, well pictured out, will be remembered by some of them.

Again, speeding him on his way to a new University position, Harper said—"Remember, when you get there, that some of the faculty members have been there five years, some ten, fifteen, twenty, perhaps twenty-five years. They probably know as much about running a University as you do. Keep your ears open, and your mouth shut!"

Dodge quotes Fairchild as saying, "he had a smile that was

irresistable and a way of looking at you that made you conscious of a presence." Fairchild thus phrased the experience of generations of students. It was an asset, not a pose. The lesser man may gradually earn profound respect from his students; Harper started with it. It did not break down with the contacts of the laboratory which soon dispose of pretensions. He could tell a student—"I don't know, that's for you to find out"—yet hold respect for his scholarship. I knew just one student at Lake Forest that never took him seriously—perhaps the most of us took him too seriously to get the best out of personal relations. Some men loom large only to the undergraduate. Harper as a leader and thinker was a figure that the student remembered with undimmed respect in the years that followed.

In 1899, he married Alice Jean McQueen who died in 1909, during his stay in Madison. After he moved to New York, he married Helen Sherman, who had at one time been in his laboratory at Wisconsin and was later in the United States Department of Agriculture at Washington. They had one son, who is a farmer at Bedford, Virginia, where Professor and Mrs. Harper moved when failing health began to appear about 1938. The Professor was a good farmer. He had owned a farm in New Jersey for years and made it a practical laboratory which kept him keenly alive to the applied side of botany.

He died at Bedford May 12, 1946. He is buried there.

We have described a man particularly fitted by nature and training to sit at one of the great botanical cross-roads of the world. Few botanists come to or go from America without passing through the Port of New York. For a quarter of a century, few failed to look for Harper at Columbia University or at the Botanical Garden. He had a prodigious grasp of problems and projects over the whole range of biology and he was always ready to meet the wayfarer upon common ground. He left us an invaluable heritage.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Amer. Jour. Bot. = American Journal of Botany
Amer. Nat. = American Naturalist
Ann. Bot. = Annals of Botany
Ber. Deutsch. Bot. Gesell. = Berichte Deutsche Botanische Gesellschaft
Bot. Gaz. = Botanical Gazette
Bot. Soc. Amer. Publ. = Botanical Society of America Publications
Bull. Torrey Bot. Club = Bulletin of the Torrey Botanical Club
Carnegie Inst. Wash. Publ. = Carnegie Institution of Washington Publications
Jahrb. Wiss. Bot. = Jahrbücher für Wissenschaftliche Botanik
Jour. Amer. Soc. Agron. = Journal of the American Society of Agronomy
Jour. N. Y. Bot. Gard. = Journal of the New York Botanical Garden
Mem. Brooklyn Bot. Gard. = Memoirs of the Brooklyn Botanical Garden
Mem. N. Y. Bot. Gard. = Memoirs of the New York Botanical Garden
Mem. Torrey Bot. Club = Memoirs of the Torrey Botanical Club
Proc. Amer. Phil. Soc. = Proceedings of the American Philosophical Society
Proc. Int. Congr. Plant Sci. = Proceedings of the International Congress of Plant Science
Trans. Amer. Micro. Sòc. = Transactions of the American Microscopical Society
Trans. Wis. Acad. Sci. = Transactions of the Wisconsin Academy of Sciences, Arts, and Letters

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H. Bateman

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HARRY BATEMAN

1882–1946

BY

F. D. MURNAGHAN

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HARRY BATEMAN

1882–1946

BY F. D. MURNAGHAN

Harry Bateman was born in Manchester, England, May 29, 1882. He was the third and youngest child of Samuel and Marnie Elizabeth (Bond) Bateman. His father, who was born in Congleton, Cheshire, was a druggist and commercial traveler. His mother was born in New York City in 1853 (her father, who came from Lancaster, having been a planter in the West Indies and America). He lived from 1884 to 1890 in Oldham, Lancashire, and his early education was received at home since, as he records, his mother did not wish him to acquire the Lancashire accent. He recounts two incidents of these early years in a manner which conveys some impression of the quiet, dry humor which was characteristic of him in later life. In order not to spoil this impression we use his own words: "One day a Mr. Pullinger, to whom my father had been apprenticed, was visiting us. As a result of some questions he had put to me he recommended me to study mathematics. I was quite impressed but my memory played me a trick when a lady asked me a few days later what I was going to study. My reply was that I was going to study acrobatics. She then asked me where I was going to perform and I was at a loss for an answer. Since I have learned recently that Dr. Thomas Young was an expert tight rope walker and harlequin my mistake does not seem so bad after all." The second story of his early days runs as follows: "Perhaps my love for the exact sciences dates from the day when I went with my sister to the home of one of her girl friends. The father of this girl was very stout and when I met him I gazed at him in astonishment and finally spoke thus: 'Mr. Booth, the next time I come to see you I am going to bring with me mother's inch tape and measure you. I think your waist line is about two yards.' 'No, Harry,' replied the good humoured Mr. Booth, 'it's nearly three'." From 1891 to 1900 he attended Board School and Grammar School in Manchester. He held Manchester City Council and Langworthy Scholarships at the Grammar School where he specialized in mathematics and ended by winning a Derby Scholarship and sizarship at Trinity College, Cambridge. When he was at Board School, and not yet twelve years old, a teacher named Arthur Gronowsky offered a prize of one shilling to the boy who was first able to demonstrate the first twelve propositions in the first book of Euclid. A shilling seemed a lot in those days to young Bateman, and he set to work to win the prize. They had good teachers then in Manchester (and, doubtless, still have) and this small piece of bread

cast on the waters by one of them certainly brought returns.

At Trinity College Bateman won a major scholarship in 1902 and took his B.A. in 1903, being bracketed Senior Wrangler with P. E. Marrack. He was Smith's Prizeman and won a fellowship in 1905 and took his M.A. in 1906. He worked very hard during his last year (1904–1905) at Cambridge for, in addition to his dissertations for the Smith's Prize and fellowship, he marked papers for the Briggs Correspondence School and coached for the Mathematical Tripos. After winning his fellowship he studied for a year on the continent, visiting Göttingen and Paris. In 1906 he was appointed Lecturer in Mathematics at Liverpool University. Before taking up this appointment he visited Professor Carey who was head of the department of mathematics at Liverpool, and he recounts that Carey's boys "besieged me with questions." At night one of them said to his father, "You told us that Mr. Bateman was a Senior Angler but he doesn't seem to know anything about fishing."

After one year at Liverpool Bateman was appointed Reader in Mathematical Physics at Manchester University. In 1910 he was appointed Lecturer in Mathematics at Bryn Mawr College where Charlotte Angas Scott, another English mathematician, was head of the department of mathematics. He spent only two years at Bryn Mawr and we can only surmise that he was not particularly successful as a teacher of young ladies or that he did not find the work particularly congenial. In 1912 he received an appointment as Johnston Scholar at Johns Hopkins University where Frank Morley, an old Cambridge mathematician, was head of the department of mathematics. The Johnston Scholarship in those days at Hopkins was a research scholarship, and the holder could give a seminar if he felt like doing so and if there were any students who felt like taking the course. Bateman had married in the summer of 1912 Ethel Horner Dodd and a son was born in 1914. The death of this son in 1917 was a blow which left its mark but which was borne with a courage which revealed the inner strength of the man. The Batemans later adopted a daughter Joan, who prepared the list of publications at the end of this memoir.

Bateman spent five years at Hopkins, holding the Johnston Scholarship for three years and being Lecturer in Mathematics for the two year period 1915–1917. In order to add to the small income from his scholarship and, later, his lectureship, he taught at the Bureau of Standards and at Mount Saint Agnes College and reviewed papers for the Weather Bureau; he also spent the summer of 1915 in Washington working for the Department of Terrestrial Magnetism.

In 1917 he was appointed Professor of Mathematics, Theoretical Physics and Aeronautics at Throop College (now the California Institute of Technology), Pasadena, California. He held this position until his sudden death from coronary thrombosis on January 21, 1946, while on the train to New York to receive an award from the Institute of Aeronautical Sciences.

Bateman's distinction as a mathematical physicist was widely recognized. He was a Fellow of the Royal Society (1928), a member of the National Academy of Sciences (1930) and of the American Philosophical Society (1924). He was vice president of the American Mathematical Society (1935) and Gibbs' Lecturer of the Society (1943). The last years of his life were devoted mainly to work connected with the war; he was a member of the War Preparedness Committee of the American Mathematical Society and chief consultant in aeronautics for the American Mathematical Society and the Mathematical Association of America.

The writer of this memoir was first brought into contact with Harry Bateman under the following circumstances. In 1914 I was awarded a Traveling Studentship in Mathematical Physics by the National University of Ireland and was looking about for some place to study. My professor, A. W. Conway, told me that there was a young man, Bateman, at Hopkins and that he thought that I could not do better than study with him. I followed this advice and, looking back over a third of a century, I judge the advice to have been sound. Bateman, a frail slight man of 32, was lecturing on The Absolute Calculus and Electrodynamics (remember that this was 1914 and that four years or more had to elapse before most of us in this country heard of Einstein's General Theory of Relativity). As I recall the situation, six students started the course and by March I was, if my memory is correct, the only student. I do not think that this diminution of the size of his class bothered the lecturer very much, and I have sometimes thought that if the vicissitudes of student life had prevented my attendance, the lecture would have been none-the-less delivered. By common standards he was not (in those early days) a good lecturer. He was too detached, too objective and perhaps too scornful of histrionic effects, and we were too untrained to profit as much as we should have from the instruction he gave us. As time went on the scene changed and he must have changed with it for I have heard enthusiastic reports of his lectures from students who took courses under him in the late twenties and thirties at the California Institute of Technology. As I think back over my two years of association (1914–1916) with him I remember well a feeling

of amazement, mingled with discouragement, which came over me when I discovered the thoroughness of the man. He already possessed a large, carefully indexed card-catalogue on each card of which was written in his minute, but beautifully clear, handwriting an abstract of a paper which he had read. I am told that in later years this card-catalogue crowded him out of his office and almost out of his home. No wonder, then, that his books and papers bristle with references which are a veritable mine of useful source material. His memory was phenomenal. No matter what stubborn integral or intractable differential equation you showed him, a moment's thought and a reference to the card catalogue never failed to produce something useful. General theories did not seem to have for him the same attraction as the special instance; the only exception to this was his devotion, which marked him as a true disciple of Hamilton, to the variational principle. As a master of the special instance I have not met his equal, nor one who approached him, and I do not think that we shall see his like again.

Bateman's best work centered around the development of the properties of special functions and the solution of the equations of mathematical physics. His first book, *Mathematical analysis of electrical and optical wave motion*, is unique and characteristic of the man. Into less than 160 small pages is crowded a wealth of information which would take an expert years to digest. Some of the material in this book may be found in expanded form in his monumental work, *Partial differential equations of mathematical physics*, published by the Cambridge University Press in 1932 and republished with minor additions by the Dover Press in 1944. This book has already taken its place beside Lamb's *Hydrodynamics* and Love's *Elasticity* as one of the classics which are part of the equipment of every worker in applied mathematics. Amongst the many results involving the functions of special importance in mathematical physics which are due to Bateman we select for special mention the formula

$$\int_0^z J_\mu(t)J_\nu(z-t)\,\frac{dt}{t} = \frac{J_{\mu+\nu}(z)}{t}, \qquad R(\mu) > 0,\ R(\nu) > -1$$

($J(t)$ being the familiar Bessel function of the first kind). This result is treated fully in Watson's book on Bessel functions and has been discussed by L. J. Mordell (J. London Math. Soc. vol. 5, pp. 203–208).

In the early years of the present century, when Bateman was a student at Cambridge, the theory of integral equations dominated the mathematical scene. Fredholm's epoch making paper (Acta Math., vol. 27) appeared in 1903 and Hilbert was actively developing the

subject at Göttingen when Bateman visited there in 1906. It was natural, then, that this new field of mathematical research should have occupied the attention of the young analyst. Papers 15, 19, 21, 22, 25, 26, 27, 28 and 42 of the bibliography below contain substantial contributions to the subject and the Report (1) to the British Association for the Advancement of Science is a valuable account of the theory as it stood in 1910. In the paper (39) Bateman applied (independently of Herglotz, who had the same idea) integral equation theory to the propagation of earthquake waves through the interior of the earth. He showed how to determine, from a knowledge of the time taken by an earthquake wave to reach various points on the surface of the earth, the velocity of propagation of the wave at various points in the interior of the earth. The full importance of this result has not yet, in our opinion, been sufficiently exploited. The velocity of propagation tells us the ratio of the appropriate elastic constant to the density. If we know this for both types of waves, longitudinal and transverse, we can determine how the density varies with the pressure. Knowing this we can set up differential equations whose solutions tell us the density and pressure throughout the interior of the earth as functions of the distance from the center. It should then be possible, from a knowledge of the variation of compressibility with temperature, to estimate the variation of temperature throughout the earth's interior.

The field in which Bateman stood preeminent was that of electrodynamics. In 1908 Hargreaves published in volume 21 of the Transactions of the Cambridge Philosophical Society a paper in which he showed that Maxwell's differential equations were merely the expression in differential or *local* form of relations between integrals over two-dimensional and three-dimensional spreads in four-dimensional space-time. Every electrical engineer knows that the relation div $\boldsymbol{B}=0$ ($\boldsymbol{B}$=magnetic induction) is merely the differential, or local, form of statement of the fact that, in magnetostatics, the flux of magnetic induction through any closed surface is zero. This integral or *global* statement is much closer to the physics of the matter than the local statement div $\boldsymbol{B}=0$. For non-static phenomena Maxwell's equations

$$-\frac{d\boldsymbol{B}}{dt} = c \operatorname{curl} \boldsymbol{E}; \qquad \operatorname{div} \boldsymbol{B} = 0$$

are merely the local form of the global statement that the flux of the magnetic induction-electric intensity tensor across any closed two-dimensional spread in four-dimensional space is zero. In the paper

(50) Bateman exploited fully this idea and showed that the group of transformations under which Maxwell's electrodynamic equations are invariant is the group of conformal transformations of four-dimensional space-time. The fundamental significance of this paper from the point of view of relativity theory was not generally recognized but Klein, in his *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, vol. 2, 1927, directs attention to this significant and pioneer work of Bateman.

This brings us to the end of the first stage of Bateman's career. In 1912 he was thirty years old, had published some 64 papers and had been two years in America. He was preparing to accept Morley's offer of a Johnston Scholarship at Hopkins (roughly equivalent to a National Research Council fellowship of today). As we look back on the situation we cannot escape the inevitable Why? Here was a man of international reputation, pleasant (if self-effacing) personality, and he had to spend the next five years in a position designed for a young unmarried Ph.D. of promise or for an established scholar on leave-of-absence or sabbatical leave. When we think of the "odd-jobs" he had to do to eke out a subsistence, the reading of papers for the Weather Bureau, the hot Washington summer at the Bureau of Standards, the teaching at Mount Saint Agnes and then recall that during this period he wrote his book on electrical and optical wave-motion, we can only subscribe to the old Latin tag: *Per aspera ad astra.*

The influence of Morley upon Bateman is shown by the publication of several papers on geometrical topics (66), (67), (68), (72). His main work, however, during the Hopkins period (1912–1917) dealt with electromagnetic theory. He was particularly interested in the lines of electric force due to a moving electron and (following J. J. Thomson) in the connection between these and the structure of the aether. During this period he wrote his book on differential equations. While this book has not generally been found well adapted to beginning courses in the subject, it well repays study, particularly by those who are more interested in the applications of differential equations to mathematical physics than in the applications to differential geometry and the theory of functions.

Shortly after his appointment in 1917 to the chair of Mathematics, Theoretical Physics and Aeronautics at Throop College (now the California Institute of Technology) Bateman published his first paper (94) on hydrodynamics, a subject which was to engage a large share of his attention during the last years of his life. However, most of his papers published during the decade 1918–1928 deal with electro-

magnetic theory. During this period his monograph on electromagnetism appeared as Bulletin No. 4 of the National Research Council (1922). In addition to his papers on electromagnetism he published a paper (97) containing an interesting application of integral equation theory to mathematical economics, two papers (106) and (119) on the numerical solution of integral equations, several papers (100), (104), (114), (116) on potential theory, papers (98), (133) and (134) on elasticity, and a paper (128) on geometry.

In 1925 Bateman was appointed a member of a committee on hydrodynamics of the National Research Council and was assigned the problem of writing those sections of a report on hydrodynamics which dealt with viscous fluids and compressible flow. The report appeared in 1932 as Bulletin No. 84 of the National Research Council. It is a large report of some 634 pages and of these Bateman's part ran to over 500 pages and is a veritable mine of information and of references to all papers of significance prior to 1932. The section on compressible fluids has been widely used and the report has been for several years out of print. Paper (136) is an important contribution to the theory of two-dimensional compressible fluid flow. The work done by Bateman on this subject during the last five years of his life is not yet available to the public due to secrecy imposed by war conditions.

Bateman was always interested in the problem of numerical computation. In collaboration with A. A. Bennett and W. E. Milne he wrote a report on the numerical integration of differential equations which was published in 1933 as Bulletin No. 92 of the National Research Council. In 1944 he published, in collaboration with R. C. Archibald, a *Guide to tables of Bessel functions* which appeared in the first volume of the journal: Mathematical Tables and other Aids to Computation.

His Gibbs Lecture, *The control of an elastic fluid*, appeared in the Bulletin of the American Mathematical Society, vol. 51 (1945) and in the same year articles by him on dynamics and elasticity appeared in the Encyclopaedia Britannica. His last paper, appropriately entitled *Some integral equations of potential theory*, appeared after his death in the Journal of Applied Physics vol. 17 (1946). We learn from a note by Professor E. T. Bell in the Quarterly of Applied Mathematics vol. 4 (1946) that he was engaged, in the time he could snatch from his war work, on "what he regarded as his most useful contributions to mathematical scholarship: an exhaustive work on definite integrals, and a critical census of all the special functions that have been considered in mathematics." It is pleasant to know that the California

Institute of Technology has invited Professor Erdélyi to prepare these works for publication.

This notice fails to convey an adequate impression of the personality of the great scientist and scholar who was lost to the world in the passing of Harry Bateman. Modest, reserved, cultured and self-effacing he was, in its true sense, a gentle man. He was very English and he must have long looked back over his shoulder, before becoming, in 1927, a citizen of the new exuberant country in which he spent more than half his life, to the quiet England of Victoria and Edward VII. He was an expert chess player and participated, when eighteen years of age, in a chess tournament between England and America. We profit from his labors and are the better for his having lived amongst us.

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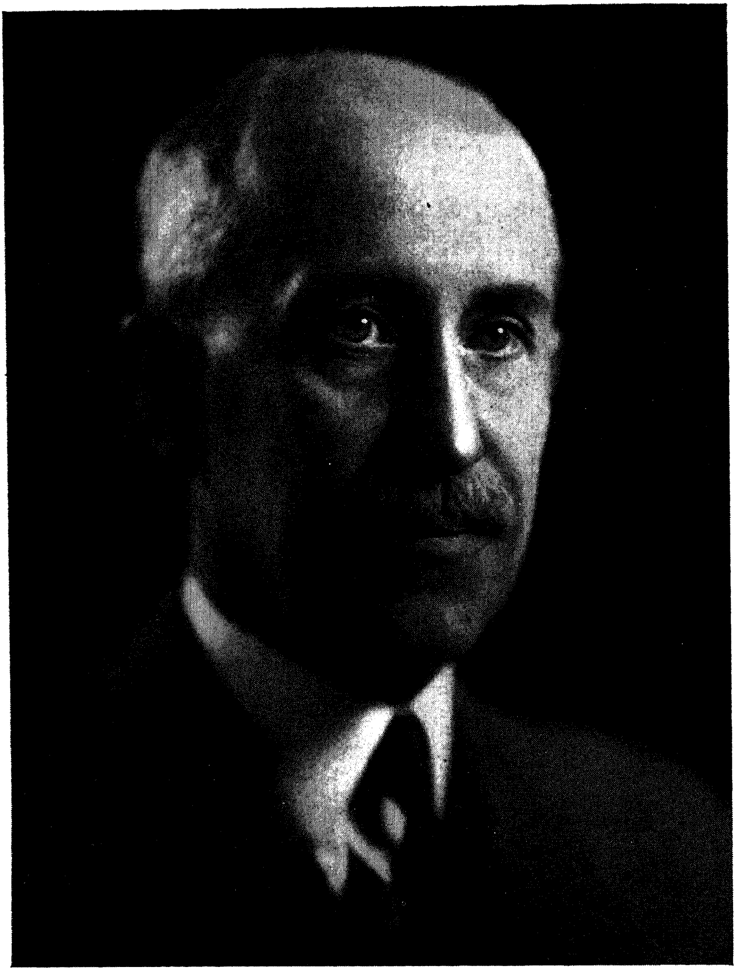
Note: In a note, No. 88, Mathematical Tables and Other Aids to Computation vol. 3 (1948) pp. 141-142, R. C. A(rchibald) calls attention to some errors and omissions in Miss Bateman's bibliography. These have to do with problems proposed or solved in The Educational Times.

F. D. MURNAGHAN



Orville Wright

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ORVILLE WRIGHT

1871–1948

BY

WILLIAM F. DURAND

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ORVILLE WRIGHT

1871–1948

BY WILLIAM F. DURAND

The boy is father to the man. Never, perhaps, has this old saying been better exemplified than in the life of Orville Wright. Born on August 19, 1871, in Dayton, Ohio, the son of Rev. Milton and Susan Catherine (Koerner) Wright, he began to show in early years the characteristics of mechanical genius and initiative which later, working with his brother, four years older, led to the successful demonstration of aerial flight in a man-made structure.

Placed in kindergarten shortly after reaching five years of age, he began the systematic evasion of the school for association with another boy to play with an old sewing machine belonging to this boy's mother. Orville watched the clock and returned home at the hour he normally would from the school. This went on merrily until his mother, seeing his teacher one day, said that she hoped Orville was doing well. The teacher replied that she had not seen Orville since the first day when his mother brought him, and supposed that she had decided not to send him to the school. What transpired when the actual facts became known is not a matter of record.

Orville was early inculcated with the lesson that if he desired spending money he must earn it and this he did in a great variety of ways. Wiping dishes, making minor household repairs and odd jobs of all sorts brought in the small sums which were expended chiefly for tools and mechanical toys.

In June 1878, when Orville was seven years old the Wright family moved from Dayton, Ohio, to Cedar Rapids, Iowa, because of the advancement of his father who was made a Bishop of the United Brethren Church. Here, soon after their arrival, the father, returning from a trip on church business showed to his boys, Wilbur and Orville, a small toy helicopter with propeller driven by a twisted rubber band, the

invention of the Frenchman, Alphonse Penaud. This toy when released, flew to the ceiling where it fluttered for a few seconds before falling to the floor. This wonderful toy made a deep and lasting impression on the boys and may be taken as the starting point of their great life interest.

Between the ages of young boyhood and early manhood, aside from progression through grammar and high school, Orville Wright occupied his time with a wide variety of enterprises and projects. Among these, the following may be noted. The organization of an army of a dozen school boys with himself as general. The life of the army was brief due to the intervention of the school janitor. In June, 1881, the family removed from Cedar Rapids to Richmond, Indiana, where Orville took up the business of building and flying kites. This soon developed into a phase of building them for sale, by which means he obtained his spending money for a time. Next came an ambitious project, the building of a foot-power full-size lathe, in which he was assisted by his brother, Wilbur. Next, with Wilbur and other boys, he organized two circuses which were carried out with parade and success, admission, one cent.

At the age of eleven Orville became interested in wood engraving and this led to the making of a printing press to make prints from his blocks. About this time the family moved again, this time back to Dayton, Ohio, where he renewed his relations with his old chum of kindergarten days, Ed Stines. Young Stines had a small printing press—not much more than a toy—and from this start there developed a long continued activity in the printing business, through the acquirement of a small but effective press by way of trade for a boat they had made. This activity passed on into the printing and issue of a small newspaper called the "Midget," together with advertising material for various clients.

By way of diversion from the printing business, Orville and his chum learned the telegraph code and with small toy senders practiced sending messages to each other, which they would later verify by shouted voice. Also, at about this time, Orville

and Wilbur undertook the somewhat extensive job of adding a front porch to the house and of making some changes in the internal arrangement of the rooms, all of which were highly appreciated by the rest of the family.

Orville's interest in printing led him to take employment during two summer vacations with a printing establishment in Dayton. Later he undertook the project of building a full size press, in which he was assisted by Wilbur. This was followed by the printing and issue of a small newspaper, the "West Side News," which ran for a year as a weekly and was then converted into a five column daily, called the "Evening News." This ran for a time and was then abandoned for a new interest, bicycles. Orville had owned, in Richmond, a bicycle of the first high wheel type. Now they found themselves in a position to buy machines of the new chain drive pattern and Orville soon became interested in track racing. This was followed by a decision to go into the bicycle business, selling, repairing, and manufacturing. Two successive moves into larger quarters followed with expanding business. Along with bicycles as the leading line, Orville found odd moments in which to interest himself in a new form of calculating machine, and also in a new and improved form of typewriter.

Starting with the toy helicopter, Orville and Wilbur continued always sensitive to anything in the public press regarding attempts at human flight. In 1895 they were much impressed with accounts of Lilienthal's gliding experiments in Germany and after his tragic death, their interest became still keener. This led to an appeal to the Smithsonian Institution in Washington for literature references on human flight. In reply several references were given and when these were obtained, it furished material for many days absorbing reading and study. Further reading and study led them to the conclusion that progress toward the solution of the problem of human flight must lie through gliding experiments. Lilienthal became their hero, and they became thoroughly absorbed in this new and enticing problem. This led, in 1899, to the building and flying of a large biplane kite fitted for a warping of the

wings by means of four cords leading to the ground. This experiment was for the purpose of testing the idea of warped wing lateral control—a control feature of the first plane to demonstrate, a few years later, the possibility of human flight. The Wright brothers were now fully committed to a serious study of the problem of human flight, first by way of gliding.

Diligent reading of all available literature on the subject was followed by the construction of their first glider in the late summer and early fall of 1900. This was first to be tested by flight as a kite. Inquiry and correspondence indicated the sand dunes of the North Carolina coast as the most suitable location for such tests and the glider was shipped there and tested during the late fall, both as a kite and, carrying Wilbur or Orville, as a glider. These tests, while encouraging, showed, in various respects, marked departures from the results they had been led to expect from such information and data as they had been able to find in their reading.

These tests of 1900 were followed by the construction of and tests on a second larger glider in the late summer and early fall of 1901. This second glider was of the same general type and form as the first, with such changes in details as their experience with number one appeared to suggest. These second tests, while still encouraging, showed many departures from what was anticipated from their available information, and presented several new problems relating to the lift and control of cambered surfaces as affected by the travel of the center of pressure on such surfaces under varying angles of attack.

In order to gain first hand information, Orville constructed a small and somewhat crude wind tunnel in which he made direct measurements of some of these puzzling questions. These results were so interesting and provocative, that the two brothers in the fall of 1901 made a much larger and more effective form of wind tunnel in which, during the fall, some two hundred forms and proportions of air foils were tested, and a large amount of useful information obtained. To put these new data to test, a third glider was built and tested in the early fall of 1902 in more than a thousand flights. These flights were very

successful and resulted in setting a number of records for gliding flights.

Following this, on their return to Dayton in the fall of 1902, they set to work on plans for a power flight. The first problem here was the engine. Failing to find a firm willing to undertake the building of an engine with the power estimated as needed and within the limits of weight available, they hired a mechanic and built the engine themselves, of 12 to 16 horsepower and, with accessories, weighing 170 pounds. The design of the propellers was a second problem which gave them much study and some trouble, all of which was successfully met, and propellers, two in number turning in opposite directions in order to balance gyroscopic effects, gave good results later in actual flight.

Finally in late September of 1903 they had everything in readiness and made their start for Kitty Hawk, N. C.

The time required for assembling the plane, installing the motor and for preliminary tests, together with spells of bad weather, carried the time along to December 17, before an actual power flight was made—the first flight in a man-made machine, carrying a man, leaving the ground under its own power and returning under control to the ground from whence it had started.

With Orville Wright at the controls, the plane on a level track, took off after a run of about 40 feet and remained in the air 12 seconds with a run equivalent to 540 feet in still air. This was followed by a second flight with Wilbur at the controls, substantially a repetition of the first flight in time and distance. Two further flights were made, the last of which with Wilbur at the controls lasted 59 seconds with a run over the ground of 852 feet.

Thus, after more than three years, given very largely to this enterprise, the possibility of human flight was fully demonstrated.

Then followed two years of intensive further development in a 68 acre flying field about eight miles from Dayton.

In January of 1904, they began the building of a new plane along the general lines of the Kitty Hawk plane, but heavier, stronger and with many changes in detail resulting from the

Kitty Hawk experience. A new engine was also built and installed. In this plane they made hundreds of flights in their field, working at improvements and changes as indicated by experience. By the fall of 1905 they had made two record flights, one of 20¾ miles in thirty-five minutes, seventeen seconds, and one of 24⅕ miles in thirty-eight minutes, three seconds.

Regarding publicity of the first flight on December 17, 1903, the brothers had desired that it emanate from Dayton, and a telegram was sent to their father briefly announcing the results. Through a leakage at Norfolk, a reporter of the *Norfolk Virginian Pilot* got a tip and published a sensational account of which the details were drawn from his own imagination. Following this, the reaction of the public at large and especially of the press seems now absolutely incomprehensible. The *Virginian Pilot* reporter sent a brief outline of the story to twenty-one newspapers in the United States with an offer of the full story. Only five were sufficiently interested to reply, and of these only three printed the story. From this time on for the next three years the Wright brothers with all of their continued experimental work adjacent to Dayton were practically ignored by the press. Editors simply refused to believe the story. Finally, however, nearly three years after the event, the *Scientific American* in its issue of December 15, 1906, printed an editorial announcement in which the editor accepted the truth of the flight and spoke of it as an "epoch making invention of the first successful aeroplane flying machine."

Curiously enough the first serious attempt to give this great news to the public was by way of a small periodical "Gleanings in Bee Culture," owned and edited by a man named Root. In several issues of his journal, Root reported on the Kitty Hawk flight and on the various flights of 1904, 1905, but these notices gained little or no recognition from the press at large. There was widespread disbelief in the possibility of human flight and a feeling of incredulity in such widely scattered references to the Wrights and their work as infrequently appeared in the press.

If the attitude of the public at large and of the press is now hard to understand, that of Government officials, especially those of the War Department is still more so. The Wright brothers from the first were very desirous of giving to the Government the full benefit of their discoveries and of their invention of a practicable flying machine. To this end they had desired to offer to our Government a world monopoly on all their patents and to impart to the proper officials all of their information regarding the design and construction of the plane.

Already England had approached the Wrights showing a definite interest in what they had accomplished and had asked them to submit a proposal to the War Office. They were in no hurry to do this, however, desiring to give their own Government a first chance. They wrote, therefore, on January 18, 1905, to their Member of Congress, outlining what they had accomplished and asking him to learn whether such performance was of interest to the Government. The letter was forwarded to the Secretary of War who, in turn, passed it on to the Board of Ordnance and Fortification.

The reply was largely a form letter in no way responsive to the Wright communication, and assumed that they had not yet brought their invention to a practicable condition.

There was some further correspondence with the British War Office, but it finally appeared that the British were more interested in obtaining information as to what had been accomplished, than in acquiring a plane for themselves. Octave Chanute, a pioneer himself in aeronautic research and a close friend of the Wrights, urged them to make another approach to the United States Government. Accordingly they drew up and sent a letter to the Secretary of War offering to build and deliver to the War Department an airplane capable of carrying an operator with a supply of fuel sufficient for a flight of 100 miles and with a minimum speed of 30 miles an hour for a distance of twenty-five miles; the plane not to be accepted until after the successful performance of these requirements.

This letter received a reply as little responsive to the proposal as did their first letter. The reply assumed that they were applicants for a grant of money for the development of the plane, and appeared to assume that the airplane under proposal was still in an undeveloped and experimental stage. This was followed by further correspondence which accomplished practically nothing in developing any basis of dealing with the Wrights, or in evoking any real interest by the Board of Ordnance and Fortification in the subject.

Finally, in the spring of 1906 Godfrey Cabot of Boston learning of this situation wrote to his relative, Senator Henry Cabot Lodge, who forwarded the letter to the Secretary of War together with a letter of his own. This all went again to the Board of Ordnance and Fortification. This resulted in a proposal to send a representative of the Board to Dayton to observe the results of the work of the Wrights, but no such representative was sent. After further desultory correspondence, the Board wrote on May 22, 1907, requesting the Wrights to make a formal proposal which was forwarded on May 31. This was followed by correspondence regarding the price and other details of the proposal, all of which resulted in nothing until some time later.

In the meantime the French were becoming deeply interested in the Wrights and in reports of what they had accomplished. Copies were made of the Wright plane and experimental work was carried on. With increase in the tempo of this interest, the Wrights became involved in a lively correspondence with France answering inquiries for information. Then followed visits in Dayton by representatives of French interests of one sort or another. Along with this keen interest in France, there was no lack of incredulity, especially among members of the Aero Club of France. With reports of visitors returning from Dayton, and with the gradual accumulation of evidence, however, the performance of the Wrights became generally accepted, and out of this grew the granting by the Wrights of an option to the French War Ministry for a limited time, for the purchase

of a plane of stated performance and at an agreed price of $200,000.

Just at this time, there was trouble brewing with Germany over Morocco, and it is believed that the interest of the Ministry was due to the desire to have an airplane available for scouting missions. In the end this resulted in nothing. The Moroccan situation became eased, it was believed the crisis was past and the Ministry failed to exercise its option.

In 1906 the Wrights were engaged in developing a new airplane engine. Their work was, about this time, brought to the attention of the Charles R. Flint Company of New York who were interested in new developments of importance, and who had connections in Europe. This developed rapidly into a proposal from Mr. Flint to act as the business representative of the Wrights, especially with reference to business in Europe. In May of 1907 a telegram came from Flint to the Wrights, urging that one of them should start for Europe at once. Wilbur was followed by Orville who arrived in Paris in August, later visiting London and Berlin. Nothing of importance came of these visits and business discussions, and Wilbur returned to the United States in November of 1907, followed a little later by Orville.

Returning from France, the Wrights found a distinctly more favorable attitude by the U. S. Board of Ordnance and Fortification and after some conference and discussion, a basis for a contract was agreed on.

The specifications called for a machine to be tested in the presence of Army officers, capable of carrying for one hour a passenger besides the pilot, with an average speed not less than 40 miles an hour in a ten mile test, and carrying enough fuel for a flight of 125 miles.

The War Department was strongly condemned in the press for asking such extremes of performance, it being asserted in the press that no such performances had as yet been demonstrated, or were indeed believed possible.

The Wright bid was accepted February 8, 1908 and construction was promptly begun. On March 3 of the same year, a

contract was signed with a wealthy Frenchman for the organization of a syndicate to buy the rights to manufacture, sell or license the use of the Wright airplane in France. With these two governments showing an active interest in the Wright airplane, the period of public disbelief in the possibility of human flight began to shift over into one of qualified belief and later into a definite acceptance of the reality of what the Wrights had accomplished.

Test flights were to be made in France and the plane for the U. S. War Department was to be tested at Fort Myer across the Potomac River from Washington. It was finally decided that Wilbur should go to France and Orville remain to carry out the tests at Fort Myer. These tests extended over a period of two weeks, some eight or ten in number, definitely establishing the fulfillment of the specifications and with continued increase of record performance. On the last flight, September 17, 1908, something went wrong with the controls and the machine crashed, killing Lieut. Thomas Selfridge, Jr., who, at his own request, was a passenger. Orville Wright himself was seriously injured, with three hip bone fractures and a dislocation of one of them.

Toward the last of December, 1908, Orville Wright recuperating from his injuries at Fort Myer, went with his sister Katherine to France to join Wilbur. The latter had been most successful in his demonstration flights and great enthusiasm was in evidence throughout France. Demonstration flights continued at Pau in the south of France, attracting great interest, among others of King Edward VII of England and of King Alfonso of Spain. After further demonstration flights in Rome, the Wrights returned to Paris and then went to London whence they returned home to Dayton.

Next came the final test flights at Fort Myer in 1909 which were successfully carried out, chiefly by Orville, resulting in substantial advances beyond specification requirements and in a corresponding bonus beyond the purchase price.

Immediately after completing these U. S. Army tests, Orville Wright set out for Berlin in accordance with an understanding

reached with German authorities on their previous visit to Europe. This visit was for the purpose of training German pilots and giving exhibition flights. The latter, given in the Tempelhof grounds and at Potsdam, were successful in the highest degree and awoke unbounded enthusiasm in the spectators. In October of 1909 he took the German Crown Prince for a flight, the first airplane flight with royalty as a passenger. His last flight in Germany was a twenty-five minute run at which Kaiser Wilhelm was an interested and enthusiastic spectator. He then sailed with his sister for New York, arriving on November 4, and thence home to Dayton.

Companies for the manufacture of Wright's airplanes had been organized in France and Germany and a plane had been sold in Italy before any serious attempt was made by American industry to undertake such manufacture. Finally, on November 22, 1909, nearly six years after the Kitty Hawk flight, a company was organized for this purpose, with many impressive names in business circles on the list of stockholders. The Wrights realized that the time was not yet ripe for manufacture on any large scale and a decision was reached to undertake the development of a more widespread interest in flying through flight exhibitions, and for a time this constituted the chief activity of the company, following which a gradual shift over into manufacture developed.

Aside from occasional exhibition flights, Orville Wright during this period devoted most of his time to the supervision of engineering at the factory in Dayton, while Wilbur was occupied in looking after patent litigation with which the Wrights were grievously plagued for some years—finally, however, winning out in the most important of these suits.

Tragedy was just ahead. On May 30, 1912, Wilbur Wright died, and Orville succeeded him as President of the Wright Company. In 1913 he made a trip to Europe on business relating to a patent suit in Germany and at the same time sanctioned the organization of a Wright Company in England.

During 1914 Orville Wright bought up all the stock of the Wright Company except that held by his friend Robert J.

Collier. This move was for the purpose of uniting the stock of the Company practically under one name and then selling out, thus retiring himself from active association with the business. This was accomplished in 1915 and his holding was taken over by a group of eastern capitalists.

Over a period of some thirty years from 1910, much of Orville Wright's attention was taken up with an unfortunate controversy with the Smithsonian Institution, finally settled to Mr. Wright's satisfaction by a statement by Dr. C. G. Abbot, executive officer of the Institution. In the years about 1910, there was correspondence between Mr. Wright and the Smithsonian Institution regarding a flying machine or a model of one for deposit in the National Museum, along with the restoration of the Langley plane which fell into the Potomac River in a trial flight nine days before the successful flight of the Wright plane at Kitty Hawk in 1903. When first exhibited in the National Museum in 1918, the Langley machine bore the simple title, *The Original Full Size Langley Flying Machine, 1903.* For this simple label, others were substituted later containing the claim that Langley's machine "was the first man carrying aeroplane in the history of the world capable of sustained free flight".

There was never any controversy as to who first *accomplished* sustained free flight. That was everywhere conceded to the Wrights, but the claim that the Langley machine was *capable* of such flight was challenged by Mr. Wright in defense of his own and of his brother's pioneer achievements. Nothing came of the early correspondence regarding the furnishing, by the Wrights, of a model or a full-sized plane for exhibit in the National Museum, but finally in 1914, resulting from a proposal by Glenn Curtis of Hammondsport, N. Y., it was decided to turn over the original Langley machine to Mr. Curtis for an attempt to prove that this machine was capable of sustained free flight.

Numerous changes were made in the machine, some of them of definite importance to the test, and with these changes, a few flights were made over Lake Keuka, N. Y., none exceeding five seconds in duration. Following this the Smithsonian an-

nounced in its Annual Report for 1914 that the Langley machine "has demonstrated that with its original structure and power, it is capable of flying with a pilot and several hundred pounds of useful load. . . ." This claim was again challenged by Mr. Wright and a long and very unfortunate controversy resulted. Finally, in 1928, Orville Wright, in reply to a request from the Science Museum, South Kensington, London, sent the original Kitty Hawk flight plane to that Institution for exhibit.

Dr. C. G. Abbot became executive officer of the Smithsonian Institution in 1928 and took up the task of carrying on negotiations with Mr. Wright regarding the matters at issue between him and the Institution. Finally it settled down to the proposal that Dr. Abbot should prepare a statement for review by Mr. Wright, covering the salient points at issue. Several drafts were made of such a statement and it was not until 1942 that one was finally drawn up acceptable to Mr. Wright. In this statement Dr. Abbot stated, "It is to be regretted that the Institution published statements repeatedly to the effect that these experiments of 1914 (at Hammondsport, N. Y.) demonstrated that Langley's plane of 1903, without essential modification, was the first heavier than air machine capable of maintaining sustained human flight."

In addition to a brief history of the controversy with Mr. Wright, the Abbot statement contained a long and detailed list of the changes made in the Langley machine, before undergoing the Hammondsport tests. With the acceptance of this statement by Mr. Wright as satisfactory to him, the way would appear to be open for the return of the Kitty Hawk machine to its native land, and to an honored place in our National Museum.

The Wrights were the recipients of many awards and medals. The list for Orville Wright includes the following: B.S. Earlham College, Indiana, 1909; LLD, 1931; Dr. Tech. Sci., Royal Technical College, Munich, 1909; LLD Oberlin, 1910; Harvard University, 1930; Huntington (Indiana) College, 1935, Sc.D. Trinity, 1915; Cincinnati, 1917; Ohio State University, 1930; M.A. Yale, 1919; Dr. Eng'g University of Michigan, 1921;

Dr. Sci. Otterbein College, Westville, Ohio, 1947; Dr. Eng'g University of Dayton, 1943; Award of Collier Trophy, 1913; Gold Medal, Aero Club of France, 1908; Aero Club of United Kingdom, 1908; Academy of Sports of France, 1908; Aeronautical Society of Great Britain, 1908; Congress of U. S., 1909; State of Ohio, 1909; City of Dayton, 1909; Aero Club of America, 1909; French Academy of Sciences, 1909; Cross of Chevalier of Legion of Honor, French, 1909; Cross of Officer of Legion of Honor, 1924; Langley Medal, Smithsonian Institution, 1910; Elliott Cresson Medal, Franklin Institute, 1914; Albert Medal, Royal Society Arts, 1917; John Fritz Medal, 1920; Bronze Medal International Peace Society; John Scott Medal, 1925; Washington Award, 1927; Distinguished Flying Cross Award, 1929; Daniel Guggenheim Medal, 1930; Franklin Medal, 1933; Hon. Member Aero Club of Sarthe France; Aeronautical Society Great Britain; Aero Club of America; Osterreichischen Flugtechnischen Vereines, Vienna; Verein Deutscher Flugtechniker, Berlin; American Society of Mechanical Engineers; Aeronautical Society of America; Institute of Mechanical Engineers, London; National Academy of Sciences; Hon. Aircraft Pilot Certificate, No. 1, issued by Civil Aeronautics Authority, 1940.

In 1915, by Act of Congress, the National Advisory Committee for Aeronautics was organized, and Orville Wright was appointed by President Wilson, one of the civilian members. This post Mr. Wright held until his death, with quite regular attendance twice a year (annual and semi-annual meetings) in Washington. He thus served as a member of this Committee for some thirty-three years.

Due to the accident at Fort Myer when Lieut. Selfridge was killed, Mr. Wright received severe back and hip injuries requiring some replacements by metal. This condition caused severe pain when traveling by railroad due to the jar and tremor of the car and his keen devotion to duty to the Advisory Committee for Aeronautics is shown by his willingness to subject himself, at least twice a year, to the train ride from Dayton to Washington for attendance at these meetings.

After his retirement from active business pursuits, Orville Wright lived quietly at his home in Dayton. For occupation he spent much time with the records of the extended research work carried on both before and following the Kitty Hawk flight; with work in his laboratory on new ideas of interest to him; and with reading. He was an active and interested reader—fiction, biography, scientific journals, with occasional turns to an encyclopedia. He often read late at night and most of his late evenings were spent in this way. His chief public obligation was attendance at meetings of the National Advisory Committee for Aeronautics. He was also greatly interested in the Dayton Art Institute and in the Engineer's Club of Dayton. Among the projects worked on in his laboratory may be noted an automatic record changer for phonograph; a cipher machine during World War II, for the purpose of speeding up the transmission of cipher messages; and an improved form of typewriter. In connection with the subject of inventions, mention may here be made of the system of airplane control by wing warping mechanically connected with rudder control, which embodies the aerodynamic equivalents of the system employed at the present time. Mention may also be made of the fact that while wind tunnels had been constructed in Europe, the wind tunnel of the Wrights was the first in the United States.

An event of supreme interest to Orville Wright occurred in December, 1928. This was a pilgrimage, organized by the National Aeronautic Association, to Kitty Hawk, to the site of the first flight in 1903, twenty-five years earlier. This pilgrimage was participated in by members of the Aeronautic Association, members of the National Advisory Committee for Aeronautics, Members of Congress, officers of the Army and Navy and other high Government officials, with, of course, Mr. Wright at the head of the list—some 200 in number.

The Congress of the United States had authorized the erection of a national monument on Kill Devil Hill by an Act signed by the President on March 2, 1927, and the National Aeronautic Association had authorized the erection of a me-

morial on the spot from which the first Kill Devil flight took place. The corner stone of the national monument was laid on December 17, 1928 at 2 P. M., by the Hon. Dwight F. Davis, Secretary of War, with appropriate addresses and the memorial of the National Aeronautic Association was unveiled on the same day at 3 P. M. with an address by Mr. John F. Victory, Secretary of the National Advisory Committee for Aeronautics. The national monument is a shaft of Monterey Granite some fifty feet in height, while the memorial is a ten ton granite boulder with a bronze tablet suitably inscribed.

Orville Wright had the reputation of never having made a public address. On two occasions, however, he made a close approach. When the two brothers returned to New York from Europe in 1909, Wilbur made a speech at a Lawyers' Club in New York and when he had finished, Orville was introduced and called on for a few words. He replied in effect that he agreed with everything that Wilbur had said, and sat down. Another occasion was when the National Advisory Committee for Aeronautics held a meeting in Orville Wright's home in Dayton on December 17, 1936. He had carefully counted the number of members and arranged the chairs in an oval form, but, by oversight, without a chair for himself. As befits a good host he stood for a time until a chair was brought. In the meantime, the regular chairman of the Committee being absent, Orville Wright was nominated to preside as chairman of the meeting; whereupon he, standing, undertook to address a seated audience arguing why he should not be elected to this office. The members noted that he was actually making a speech, which caused him to abruptly terminate his remarks, whereupon he was duly elected temporary chairman.

In April of 1946 the President signed the certificate of the Award of Merit to Orville Wright for distinguished service with the National Advisory Committee for Aeronautics during the great war. Due to delays in the War Department, plans for the actual award were delayed until January of 1948 and it was planned that he would come to Washington on January 15 for that purpose, but the condition of his health would not

permit and gradually growing weaker, he died on January 30, without personally having received the award.

The name of Orville Wright will always stand as representing a man of fine personal qualities, modest yet firm in holding to what he considered right and just, and withal a great and original thinker and with his brother, the first to solve the problem of human flight in a man-made machine, with all that this epoch making invention has developed into in our own day.

LIST OF PRINCIPAL CONTRIBUTIONS TO SCIENCE BY ORVILLE WRIGHT

Stability of Aeroplanes. *Franklin Institute, Journal,* vol. 178, pp. 249-258, September, 1914. Published also in *Annual Report of the Board of Regents of the Smithsonian Institution,* 1914, pp. 209-216. Abstracted in *Scientific American Supplement,* vol. 78, pp. 206-207, September 26, 1914. Presented at the stated meeting of the Institute held Wednesday, May 20, 1914, when Dr. Wright received the Institute's Elliott Cresson Medal.

How We Made The First Flight. *Aviation,* v. 15, pp. 737-741, December 17, 1923. Reprinted from *Flying,* December, 1913.

The Early History Of The Airplane. Dayton, Ohio, [1922]. (Pamphlet.) Contains three articles, which are reprints of articles in the *Century Magazine* and *Flying.* pp. 1-8. The Wright Brothers' Aeroplane, by Orville and Wilbur Wright, pp. 9-15. How We Made The First Flight, by Orville Wright. Dayton-Wright Airplane Co.

(With Wright, Wilbur). Our Aeroplane Tests At Kitty Hawk. *Scientific American,* vol. 98, p. 423, June 13, 1908.

(With Wright, Wilbur). The Wright Brothers' Aeroplane. *Century Magazine,* vol. 76, pp. 641-650, September, 1908. Reprinted in *Aviation,* Vol. 15, pp. 732-737, December 17, 1923.

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Margaret Floy Washburn

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BIOGRAPHICAL MEMOIR

OF

MARGARET FLOY WASHBURN

1871–1939

BY

ROBERT S. WOODWORTH

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1948

MARGARET FLOY WASHBURN

1871–1939

BY ROBERT S. WOODWORTH

The Harlem district of New York City, in 1871, still had the characteristics of a roomy residential suburb. "Both sides of 125th Street from Fifth Avenue to the Hudson River were occupied by white-painted frame mansions set in gardens." A house near Fifth Avenue stood in a large garden, part of a tract of several acres which had been acquired about 1800 by Michael Floy. This English-born great-grandfather of Margaret Floy Washburn was the last of her ancestors to arrive in this country, the others—Dutch, Flemish, English, Welsh, Scottish—having previously settled in New York, Maryland, Virginia and Connecticut. Michael Floy built up in Harlem a profitable business as florist and nurseryman. Others of her ancestors and senior relatives were druggists, teachers, clergymen, and one woman physician. Literary and musical tastes were shown by many in this large family group, though not professionally.

Dr. Washburn looked back with affection on her Harlem birthplace and childhood home. "It seems to me that my intellectual life began with my fifth birthday. I remember a few moments when I was walking in the garden; I felt that I had now reached an age of some importance, and the thought was agreeable. Thinking about myself was so new an experience that I have never forgotten the moment." Her father was in business at this time but a little later became an Episcopal minister serving parishes upstate in Walden and Kingston. He was a man of intellectual tastes but uncertain temper. Her mother's nature, she said privately, was perfectly balanced with natural strength and sweetness of character, a fine mind and musical talent. Her own early schooling was anything but regular. She learned to read at home at an early age and read a great deal, "enjoying the blessed privilege of an only child to be undisturbed when at leisure." She attended small private schools for three or four years, and a public school for a year

or more before entering high school at the age of twelve. She entered Vassar College at the age of sixteen. Her love for literature she continued to foster by private reading, while her intensive college work was in chemistry, biology, and philosophy. She graduated from Vassar in 1891.

"At the end of my senior year I had two dominant intellectual interests, science and philosophy. They seemed to be combined in what I heard of the wonderful new science of experimental psychology. Learning of the psychological laboratory just established at Columbia by Dr. Cattell, who had come . . . from the fountain-head, the Leipzig laboratory, I determined to be his pupil. . . . But Columbia had never admitted a woman graduate student: the most I could hope for was to be tolerated as a 'hearer.' . . . Dr. Cattell treated me as a regular student and required of me all that he required of the men. . . . At the end of the year . . . he advised me to apply for a graduate scholarship at . . . Cornell. I feel an affectionate gratitude to him, as my first teacher, which in these later years I have courage to express; in earlier times I stood too much in awe of him. While I was thus being initiated into Cattell's objective version of the Leipzig doctrine, the influence of William James's *Principles* was strong. . . . I went in the fall of 1892 to Cornell, where Titchener had just arrived from Oxford and Leipzig." [1]

During her first year at Cornell she was Titchener's only major graduate student. The second year she was joined by Walter B. Pillsbury. Titchener, while ably representing Wundt's introspective experimental psychology, had not yet worked out his own rigidly "structural" or "existential" point of view, to which indeed these two earliest students of his never subscribed though they fully believed in the value of introspective methods. Pillsbury (2) "remembers Miss Washburn well from the Cornell period. . . . She was a brilliant conversationalist, inclined to be rather acid in her comments on men and things. Her keen sense of humor was fully developed at this time. . . . Titchener had not formulated his structuralism. . . . His main aim was to establish psychology as a science. . . . When the more rigid system developed, Miss Washburn

[1] This and the other quotations so far are from Dr. Washburn's autobiography (1932).

showed a lack of sympathy with the more extreme tenets." Her research in both the Columbia and the Cornell laboratories was devoted to the perception of distances and directions on the skin, with an added original emphasis on the part played by visualization in these cutaneous perceptions. Her dissertation on this problem had the honor (at the time a distinct honor) of being accepted by Wundt for publication along with the output of his own laboratory (1895). Her studies at Cornell were in philosophy as well as psychology, and on obtaining the Ph.D. degree in 1894 she "gladly accepted the Chair of Psychology, Philosophy, and Ethics (not to mention Logic)" at Wells College, where she remained six years. With two additional years back at Cornell as "warden" of a students' residence hall and (incidentally) lecturer on social and animal psychology, she passed in all ten years in the neighborhood, making much use of the Cornell laboratories and libraries, and keeping in touch with the philosophers as well as the psychologists. The position of warden with its time-consuming social functions and its responsibility for the behavior of the girl students proved to be highly uncongenial, and she was glad to obtain a position at the University of Cincinnati as assistant professor in full charge of psychology. She was still more pleased a year later, in 1903, to receive a call to a similar position at her alma mater, Vassar College, where she remained the rest of her life.

Professor Washburn was eminently successful as a teacher and administrator. She soon built up one of the strongest undergraduate departments of psychology in the country. "Her lectures were brilliant, exact, clear, with such a wealth of references and citing of original sources as almost to overwhelm a student as yet unable to appreciate the breadth of the scholarship and the painstaking labor involved in the construction of a single lecture" (8). She was not merely an ardent and thorough scientist: she was a vivid personality with great influence on her students, colleagues and fellow psychologists. "The key to her personality was a unique attitude, in which were combined a detached objective devotion to experimental science and a passionate joy of living. She practiced with keen appreciation the arts of painting, music, the theater, and the dance.

Her studies of the animal mind were inspired by the quality of temperament expressed in her last words spoken in health, 'I love every living thing.' In especial, she loved and stimulated her pupils" (7). Many of her students went on to graduate study in various universities and to careers in psychology. She never attempted to develop graduate study at Vassar, since she deprecated such study for women at any but coeducational universities.

A very successful educational venture which she introduced at the beginning of her teaching at Vassar consisted in collaborating with her major students in compact and well-defined pieces of research.

"In order to give the senior students in psychology a glimpse of research methods, a few simple experimental problems were devised each year, whose results, if they worked out successfully, appeared in the *American Journal of Psychology* as 'Studies from the Psychological Laboratory of Vassar College.' The problem and method of a study having been determined by me, the experimenting was done by the students, who also formulated the results; the interpretation and writing of the reports fell to me and the paper was published under our joint names" (6).

Her bibliography (4, 5) contains nearly seventy of these joint papers. They are brief and to the point but many of them are substantial scientific contributions. Considered as a device for continued productive research by a busy teacher, the plan worked out very well. Though quite a variety of problems were attacked, the research as a whole cannot be called scattered, since it revolved about a few persistent problems of the major investigator.

Spatial perception by the different senses was one recurring problem in her experimental work and that of her students. Early work of this kind on the skin sense was followed later by studies of the perception of the third dimension by the eye, with special reference to binocular rivalry and other forms of fluctuating perception. After-images were a related field of work.

Memory for hand movements and other movements in space was a topic of interest. And memory for emotional experiences

was especially interesting to her students. When a person was asked to revive as strongly as possible some past experience of fear or anger or joy, it was found that joy was more strongly and also more quickly brought back than fear or anger. "Anger is frequently felt, but . . . reluctantly recalled."

Experimental esthetics was one major enterprise of the Vassar laboratory. A simple but effective rating method was employed in a large variety of studies on the esthetic effect of pictures, music, colors, and speech sounds. Of the vowels, the sound of *u* as in *mud* was rated the least pleasant, while *e* as in *get* and *a* as in *father* were the most pleasant vowels though less pleasant than *l, m* and *n*. These preferences were traceable partly but not wholly to associations. More pronounced were color preferences. The pleasure of a color combination was found to be due only in part to the pleasantness of the colors individually, since two pleasant colors placed side by side might make an unpleasant impression, and even a pair of unpleasant colors might give a pleasant effect. There were many other relevant findings on fatigue, fluctuation, habituation, suggestion, and voluntary control of one's likes and dislikes. The law of "affective contrast"—that moderately pleasant colors, for example, become more pleasant when interspersed with unpleasant colors, less pleasant when interspersed with very pleasant ones—was first demonstrated in the Vassar laboratory.

The detection and measurement of individual differences constituted another major enterprise. Tests of freshmen for predicting scholarship in college were tried out and some good ones identified. A simple test for retaining spatial relationships was fairly indicative of aptitude for geometry. In the difficult field of emotional and temperamental traits Dr. Washburn did much pioneering. Was it possible by laboratory methods to distinguish the excitable from the phlegmatic individual, or the optimistic from the pessimistic? Promising leads were opened up though conditions were not favorable for the large-scale sampling required in the standardization of such tests for general use.

Finally, a few of these Vassar Studies dealt with problems of animal psychology. Color vision, i.e., the ability to distinguish between light of different wave lengths, was demonstrated in certain fishes and disproved in the rabbit. This problem is much more difficult than it seems but she was aware of the pitfalls and her conclusions have held good. Other animal studies were concerned with problems of motivation and orientation.

Her interest in animal psychology was rooted in an intense love for animals. She wished to learn as much as possible about their conscious experience and not merely about their external behavior. Admitting that no logical demonstration of conscious experience in animals was possible, she still believed it worth while to consider the nature and limitations of such experience, provided it was present at all. For example, since the rabbit shows no power of discriminating red from green, we may safely draw the negative conclusion that these color sensations are absent from this animal. Since some fishes do discriminate in their behavior between light of different wave lengths, we may safely infer that they get different sensations from the different wave lengths—provided, that is, we assume the fish to have any sensations at all. This inference is somewhat weakened, to be sure, by the absence from the fish's brain of a cerebral cortex which in man appears to be necessary for conscious experience. The more similar the anatomical structure, as well as the behavior, of an animal to man, the more confident we feel in assuming conscious experience in the animal.

"Our acquaintance with the mind of animals rests upon the same basis as our acquaintance with the mind of our fellow man; both are derived by inference from observed behavior. The actions of our fellow men resemble our own, and we therefore infer in them like subjective states to ours: the actions of animals resemble ours less completely, but the difference is one of degree, not of kind. . . . The mental processes in other minds, animal or human, cannot indeed be objectively ascertained facts; the facts are those of human and animal behavior; but the mental processes are as justifiable inferences as any others with which science deals. . . . We know not where

consciousness begins in the animal world. We know where it surely resides—in ourselves; we know where it exists beyond a reasonable doubt—in those animals of structure resembling ours which rapidly adapt themselves to the lessons of experience. Beyond this point, for all we know, it may exist in simpler and simpler forms until we reach the very lowest of living beings." [2]

The point of view thus clearly expressed in 1908 in the first edition of Dr. Washburn's book, *The Animal Mind,* was maintained in the later editions of 1917, 1926, and 1936. Meanwhile, beginning about 1912, the radically different "behavioristic" point of view became prominent and influential. The behaviorists argued that since "the facts are those of human and animal behavior," the theories also should remain at the behavior level, and that the whole of psychology should treat of behavior only with no reference to conscious experience. She regarded this scrapping of the extensive knowledge gained by introspective methods in the study of human sensory processes as a wasteful and essentially stupid procedure and as one which would rob animal psychology of most of its interest and fruitfulness. For it is by reference to our own experience that we are able to interpret animal behavior in a meaningful way, and it is by use of our own experience that we can formulate fruitful hypotheses to be put to the necessary behavioral test of experiment. Such visual phenomena as after-images and flicker are known at first hand in our own sensory experience and can be used for setting up hypotheses to be tested in animal behavior. Even John B. Watson's famous theory of human thinking as consisting of minute speech movements was probably suggested by the everyday introspective observation of silent speech in thinking. Such was Dr. Washburn's argument in her presidential address before the American Psychological Association in 1921. A dualism of physical and mental processes seemed to her inescapable, no matter how strongly one might prefer a monism. Red as a physical stimulus has a certain wave length, but the sensation of red has no wave length. Heat as a physical stimulus is a mode of motion, but the sensation of warmth is something

[2] From M. F. Washburn: *The Animal Mind.* Copyright, 1908, by The Macmillan Company and used with their permission.

entirely different from motion. In their zeal for a physical monism the behaviorists were proposing to deny the existence of sensations or at least to regard them as of no possible scientific interest. Miss Washburn regarded sensations and other conscious experiences as both real and important for science.

In *The Animal Mind* the problem of consciousness in animals recurs in relation to the different senses, space perception, memory, problem solution, etc., but does not by any means dominate the book. The book is, rather, a comprehensive survey of animal behavior, based on a critical analysis of the literature which even in 1908 demanded a bibliography of 476 titles, increasing from edition to edition up to a total of 1683 titles. This pioneer treatment of the subject was influential in the development of animal psychology. In its successive editions can be found many judicious interpretations and ingenious suggestions, some of which are still awaiting the attention they deserve. How do ideas, as distinct from sense perceptions, first arise? Her suggestion (1908, p. 273) was that they probably arose as anticipations of what was about to happen rather than as memories of what had happened. When is the drive toward a goal strongest? She suggested (1936, p. 379) that it reached its peak just as the goal was on the point of being attained, so that the successful act which reaches the goal is done with the strongest drive and therefore becomes most strongly associated with the drive. Here we have the germ of a possible future theory of learning, a theory which would apparently avoid most of the pitfalls of existing theories.

One of her theories which she worked out most thoroughly was a motor theory of ideas and of perceptions as well. In the presence of an object we have a sensory impression of it; we get the look of it, the sound of it, or the feel of it. In the absence of the object we may be able to revive this sensory impression so as to remember how it looks, sounds, or feels. According to the older theory, our idea of an object consists of these revived sensory impressions. But we are motor creatures as well as sensory. There is something more to a perception than a sensory impression; there is an incipient movement, a readiness to approach and manipulate the object, or per-

haps to avoid it. Different objects are manipulated differently, and the motor theory holds that each perceptibly different object awakens a different motor readiness. Similarly, the idea of an object is more than a revived sensory impression, for it involves an incipient movement, a readiness to manipulate the object in the characteristic way. Motor readiness can easily be demonstrated in animals—as in the dog ready to chase a ball as soon as you throw it—but whether animals have the power of reviving sensory impressions is very doubtful. So it would seem that the motor element in an idea is more primitive and fundamental than the sensory element. The power of reviving sensory impressions may well be a derivative of the power of making anticipatory movements. Motor anticipation may give rise to sensory anticipation. Consider an animal ready to execute a certain movement but forced to wait for a starting signal—to wait for the ball to be thrown—nerve energy may overflow from the motor to the sensory cortical areas and produce a sensory experience, an idea of what is about to happen. Ideas would arise, then, when motor activity is partially blocked.

Learning, according to this theory, is primarily a motor affair and consists in the association of movements into regular series and simultaneous combinations, the association taking place by way of the muscle-sense stimuli produced by the contraction of the muscles. When two movements are made in quick succession, the muscle-sense stimuli produced by the first movement reach the brain in time to play a part in the innervation of the second movement, and thus the two movements become chained into an integrated sequence, as in the pronouncing of a two-syllable word. Much more elaborate movement systems are developed by an extension of this process. Ideas, being in part motor affairs, are associated in the same way. A group of ideas becomes an organized system by the integration of their motor components. Incompatible ideas are such as demand incompatible movements. The enormous number and variety of human ideas are provided for by the variety of possible movements of the hands, eyes and speech organs. Meanwhile the large muscles of the trunk and limbs play their part in thinking, for it is they that assume the postures of ex-

pectancy, doubt, questioning, acceptance or rejection, and persistent purposive activity.

This motor theory was foreshadowed in some of her early papers, worked out with great care in her book of 1916, clearly and briefly presented in her two papers of 1928, and outlined in her contribution to the collection, *Psychologies of 1930*. As she explains in this last article, some such physiological theory seems to be demanded by the nature of scientific thought (p. 81):

"So far as we can comprehend it, the world involves two types of processes: (a) material processes . . . and (b) mental processes. . . . The material world is a sum of movements, but no sensation quality can ever be identified with a movement. Blue may be caused by movement of a certain frequency, but it is not itself a movement. . . . The world of qualities or conscious processes never affects the world of movements or material processes causally. . . . It is only a movement or material process that can cause or in any way influence another material process."

To which may be added a sentence from 1928 (p. 104):

"While consciousness exists and is not a form of movement, it has as its indispensable basis certain motor processes, and . . . the only sense in which we can explain conscious processes is by studying the laws governing these underlying motor phenomena."

In spite of her persistent labors on this theory Miss Washburn was at heart an experimentalist. "The results of experimental work," she said in her autobiography, "if it is successful at all, bring more lasting satisfaction than the development of theories." She was an active member of the Society of Experimental Psychologists, whose custom it is to get together in informal round-table discussions of research in progress. With a Vassar colleague she won the Edison prize for an experimental study of "the emotional effects of instrumental music." Her long list of experimental studies has already been mentioned.

She also took an active part in organizational matters, being an excellent collaborator and committee member. Many honors and responsibilities came to her from her psychological colleagues. She served at different times as President of the

American Psychological Association, as President of the New York Branch of that association, and as Vice-President of the American Association for the Advancement of Science and chairman of its psychological section (Section I). Twice, in 1919-1920 and again in 1925-1928, she was a member of the Division of Anthropology and Psychology of the National Research Council. As chairman of an American Psychological Association committee and of a National Research Council committee she played a leading role in inaugurating the valuable journal, *Psychological Abstracts*. She carried editorial responsibilities in several journals, especially the *American Journal of Psychology* which she served for thirty-six years. Her colleagues on the journals, in recognition of her eminent services, joined in 1927 in presenting to her a Commemorative Volume of the *American Journal of Psychology*. A woman of great personal charm she also possessed in high degree the desire and ability to collaborate on terms of perfect equality with all colleagues, male and female, young and old. For three decades an active and productive member of the scientific fraternity, she well deserved the honor of membership in the National Academy of Sciences.

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MARGARET FLOY WASHBURN: VITA

Born in New York City, July 25, 1871. Died in Poughkeepsie, N. Y., October 29, 1939.

Vassar College: A.B., 1891; A.M., 1893. Cornell University: Ph.D., 1894. Wittenberg College: D.Sc., 1927.

Wells College: Professor of Psychology, Philosophy and Ethics, 1894-1900.

Cornell University: Warden of Sage College, 1900-1902; Lecturer in Psychology, 1901-1902.

University of Cincinnati: Assistant Professor in charge of Psychology, 1902-1903.

Vassar College: Associate Professor in charge of Psychology, 1903-1908; Professor of Psychology, 1908-1937; Professor Emeritus, 1937-1939.

American Psychological Association: Member of Council, 1912-1914; President, 1921.

American Association for the Advancement of Science: Vice-president and Chairman of Section I, 1926.

National Research Council, Division of Anthropology and Psychology: Member, 1919-20, 1925-1928.

National Academy of Sciences: Member, 1931-1939.

New York Branch of the American Psychological Association: President, 1931-1932.

Society of Experimental Psychologists: Chairman, 1931.

International Congress of Psychology: Member National Committee, 1929; Member International Committee, 1929-1939.

National Institute of Psychology.

New York Academy of Sciences.
American Philosophical Society.
Phi Beta Kappa.
Sigma Xi.
American Journal of Psychology: Cooperating Editor, 1903-1925; Co-editor, 1926-1939.
Psychological Bulletin: Cooperating Editor, 1909-1915.
Psychological Review: Advisory Editor, 1916-1930.
Journal of Animal Behavior: Associate Editor, 1911-1917.
Journal of Comparative Psychology: Associate Editor, 1921-1935.
Dictionary of Psychology: Advisory Board, 1934.

KEY TO ABBREVIATIONS

Amer. J. Psychol. = American Journal of Psychology.
J. Animal Behav. = Journal of Animal Behavior.
J. comp. Neurol. Psychol. = Journal of Comparative Neurology and Psychology.
J. comp. Psychol. = Journal of Comparative Psychology.
J. Phil. = Journal of Philosophy, Psychology and Scientific Methods.
Phil. Rev. = Philosophical Review.
Phil. Stud. = Philosophische Studien.
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences.
Psychol. Bull. = Psychological Bulletin.
Psychol. Rev. = Psychological Review.

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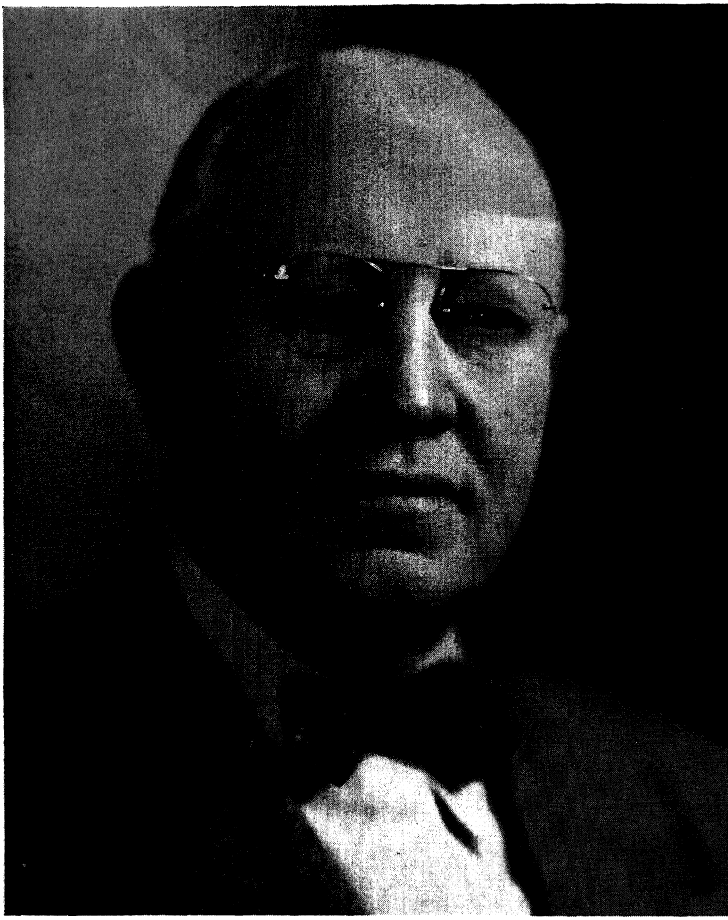

NATIONAL ACADEMY OF SCIENCES

OF THE UNITED STATES OF AMERICA

BIOGRAPHICAL MEMOIRS

VOLUME XXV—THIRTEENTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

GEORGE WILLIAM LEWIS

1882–1948

BY

WILLIAM F. DURAND

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1949

GEORGE WILLIAM LEWIS

1882–1948

BY WILLIAM F. DURAND

The crisis brings forth the man. The significance of aircraft as a means of warfare began to be recognized during World War I. Once the war was over there began to be an insistent demand, on the part of the armed forces, for large expansion of various types of aircraft, both military and naval. This was brought into some form of symmetry and reason by the Coolidge Air Board of 1925.

In the meantime, ten years earlier, in 1915, the Congress had created the National Advisory Committee for Aeronautics with the specific mission "to study the scientific problems of flight with a view to their practical solution."

This body, as then organized, comprised two representatives from the Army, two from the Navy, one representative each from the Smithsonian Institution, the Bureau of Standards and the Weather Bureau, together with five civilians, selected with reference to their training and professional record. The appointments to membership on the Committee were made by President Wilson and the new organization began actively a survey of its problems.

From the first it became evident that progress in the development of new and improved types of aircraft was definitely conditioned on a greatly augmented understanding of the phenomena of aerodynamics, of internal combustion engines and of the materials of aircraft construction. This implied a wide and intensive program of research.

On request the Army, during World War I, had allotted to the Committee an area in its Langley Field, near Old Point Comfort, Virginia, for the construction of a research laboratory. A start had been made comprising an administration and general purpose building, a small wind tunnel with building, an engine research building or shed, and a hangar loaned by the Army with two planes for experimental purposes.

A leader was needed to direct the work of this laboratory, and in 1919 the choice fell on George W. Lewis, a young employe

of the Committee since 1917, whose initiative, energy, tact and winning personality had already strongly impressed all who had come in contact with him.

This was the crisis, and this was the man who appeared, ready and able to undertake the serious responsibilities of the situation.

George William Lewis was born in Ithaca, New York, March 10, 1882, the son of William Henry Lewis and Edith Sweetland Lewis. From Scranton High School he went to Cornell University, graduating in 1908 as a mechanical engineer. He remained at Cornell for two years as Instructor and graduate student, receiving his Master's degree in 1910. He then joined the faculty of Swarthmore College, and in 1917 became Engineer-in-Charge of the Clarke Thompson Research of Philadelphia.

His first contact with the Committee, or the N.A.C.A., as it soon came to be called, was in the summer of 1917 when he appeared at the office of the Chairman, in the Munsey Building, Washington, D. C., with a proposal for a research in connection with the two cycle type of internal combustion engine, which he thought might be of interest to the Committee. The Chairman recognized promise in the project and arrangements were made for carrying it on as a Committee undertaking, with young Lewis in charge.

His handling of this project, together with other matters assigned to him, resulted in 1919, as noted above, in his appointment to take charge of the Committee's laboratory at Langley Field, an appointment which in 1924 was changed to Director of Research for the Committee.

At the time of his appointment in 1919 to take charge of the laboratory, it was only a small organization of some 20 employes, with buildings and equipment as noted earlier. Its growth down through the years has been impressive in the highest degree.

Lewis early recognized that sound progress in aeronautic science and engineering required the fulfillment of four conditions—an adequate understanding of the aerodynamic phenomena involved in flight, a like understanding of the operation of internal combustion engines as a means of propulsion, a

thorough knowledge of the structural materials available and best suited to aircraft construction, with an adequate knowledge of the stresses to which they would be subjected in flight, and finally a definite correlation between the conditions of flight performance as measured in the laboratory, model or full scale, and the actual conditions of flight in free air.

As time went on and he became more effectively adjusted to his job, he advanced boldly and strongly with proposals for new, larger and faster wind tunnels, tunnels for special purposes—for the study of stability and control, for the study of turbulence, for the elimination or reduction to a minimum of the turbulence in the tunnel air stream, thus approaching the conditions of flight in the open air; for studies of gusts and their effect on stability and control, for studies on ice formation and means for its prevention; supersonic wind tunnels for pushing into that great unknown domain lying beyond speeds equal to that of sound in the air through which the plane is flying; equipment for the analysis of engine and propeller performance, combustion and fuels, lubrication and friction, balance and vibration, the materials available for aircraft construction with refined and carefully planned equipment for the study and analysis of stresses and strains in aircraft structures.

And then there was the Instrument Division which concerned itself with the invention, design and construction of a whole line of new and special instruments and appliances for making observations on and measurements of aerodynamic and combustion phenomena.

The proposals made by Lewis for realizing these various purposes met with the approval of the Committee and gained the support of Congress, and the plot of ground originally allotted to the Committee was soon fully occupied with buildings housing new and highly specialized equipment adapted to the study of aerodynamic and combustion phenomena.

With this area fully occupied, a far lying corner of the field was next taken over for extension, and here a considerable number of new buildings was erected and equipment provided, each again intended to meet some special phase of aeronautic research.

Lewis was a pioneer in the design, construction and use of variable density, full scale, refrigerated, free flight, gust and high-speed wind tunnels.

Dr. Lewis lived to see seventeen wind tunnels in operation at Langley Field, of varied dimensions, forms and air speeds, and intended for the exploration of almost every form of aerodynamic problem subject to investigation by such means.

Dr. Lewis found it necessary to concern himself not only with aircraft in the narrow sense of the term, but also with seaplanes and flying boats as well. With such craft there is the water-borne phase and the air-borne phase. The latter could be investigated by the use of the same facilities as for normal aircraft. But for the former, special facilities were required and under his direction there were designed and constructed at Langley Field two seaplane channels, one having a length at first of 2020 feet and placed in operation in May of 1931, later extended to a length of 2960 feet and placed in operation in October of 1931 and a second special purpose channel of length 1800 feet and placed in operation in December 1942.

Here have been carried out long series of investigations and researches relating to the water-borne phases of such craft. They have been of the greatest importance in the development and improvement of such craft and constitute definitely one of the great contributions to aero-hydro science standing to the credit of the N.A.C.A.

The supply of electric power available to meet the demand for power drive at Langley Field was limited and this caused a limitation to needed further expansion. In consequence, in the late years of the decade 1930-40, a serious study was undertaken of the question of another laboratory site away from Langley Field and preferably in the west or on the Pacific Coast. After long and careful investigation, the choice fell on Moffett Field, near Palo Alto, California. The location of the new site at this point resulted from its meeting, adequately, the requirements of the supply of electric power, climate favorable to all-the-year flying weather, and nearness to the great centers of aircraft industry in southern California.

Here a start was made on the construction of laboratory build-

ings and the supply of equipment in the closing years of the decade, and in June of 1944 the Ames * Aeronautical Laboratory was there opened and dedicated to the service of our country, and more widely to the world-wide domain of aeronautic research.

Here is the mammoth full scale tunnel with a throat opening of 40 by 80 feet, a 6 foot by 6 foot supersonic wind tunnel, with eleven others of throat diameters up to 20 feet, and each intended for special phases of aeronautic research.

In the spring of 1939 Lewis made a trip to the chief centers of aeronautic research in Europe. He was especially impressed with the character and extent of the work which he saw there directed toward improvement in the aeronautic engine. These impressions strengthened and confirmed his own feelings on this subject, and on his return to Washington he recommended most strongly to the Committee the development of a laboratory for like purposes in the United States. This important step by Lewis was a natural expression of his earlier training. He was a mechanical engineer by education and naturally sensitive to the part which the aeronautic engine was then playing as the agency for the supply of power for the propulsion of the plane.

He carried the Committee with him in his recommendation and a committee was appointed to consider the question of site. After a careful study of some 72 proposed locations, a site in Cleveland, Ohio, was selected as having the best combination of the following features—proximity to a large flying field; proximity to industrial, technical and scientific centers; living and working conditions; availability and cost of power and water services, and area available for future expansion.

Here in 1940 a start on construction was made and in 1942 the laboratory was opened and work on a wide variety of problems relating to the internal combustion engine for aeronautic purposes was promptly under way.

Here are to be found, among six wind tunnels of varying sizes and wind speeds and for a variety of purposes, an altitude tunnel capable of simulating flight conditions as regards air

*Named in honor of Dr. Joseph S. Ames, President of Johns Hopkins University, and Chairman of the N.A.C.A. for 12 years.

density and temperature up to altitudes of 50,000 feet and where aeronautic engines can be studied in operation under accurately controlled conditions with air stream speeds up to 500 miles per hour.

An 8 foot by 6 foot supersonic tunnel, believed the largest supersonic wind tunnel in the world, is located here. There is also an icing tunnel for research on the icing of aircraft components.

It is appropriate to add at this point that, at a later time, the Committee gave to this laboratory the name "Lewis Flight Propulsion Laboratory," in honor of Dr. George W. Lewis and in recognition of the part which he played in its design and construction.

At each of these laboratories a special feature was made of a Flight Research Division. Lewis held it as a cardinal principle that laboratory results, especially if to model scale, before being turned over for use in the armed forces or in industry, must be proven in the air. In accordance with this principle, at each of the three laboratories he directed the organization of a strong flying division with skilled pilots, scientists, engineers and technicians, for the final checking of laboratory results under the conditions of actual flight in the air.

This required the invention, design and construction of a wide variety of special instrumental equipment for making and recording the observations while in the air. This special activity, it may fairly be said, has led the world of aeronautic research, especially in actual flight, and it traces immediately to Lewis and his strong and enthusiastic support of the instrument division in the various laboratories.

An important feature of the activities of the N.A.C.A. was the policy, fostered by Lewis from the first, of research contractual relations with technical institutions of higher learning. From his background, both as educator and research worker, he was in a position to realize clearly the mutual advantages to be gained by cooperative relations with technical educational institutions. The Government gained the services of talented personnel, not otherwise available, and often of specialized equipment. The institution gained in the extension of its range

of problems for advanced work and in a sense of partnership with the Government in dealing with its technical and scientific problems. A further benefit resulted to the Committee from the interest awakened in bright young students through contact with aeronautics problems, an interest which furnished a field for the selection of additions to laboratory personnel.

The relation was worked out on the basis of a simple form of contract calling for a report at a specified time on a carefully defined problem. This program of educational contracts has grown in magnitude and importance from an annual total of some $5,000 for four or five contracts to an annual total of $600,000 for some 35 contracts with the same number of educational institutions. In all a total of some 400 contracts with 50 institutions have produced results of great importance and of definite value. As a less tangible benefit, these contracts have served to bring more closely together teaching staffs and students in aeronautic engineering on the one hand with Government personnel engaged in aeronautic research on the other—undoubtedly to the advantage of both.

A cardinal feature of the Lewis policy in carrying on the work of the Committee was his controlling sense of obligation to serve the needs of the armed services. From the first he developed cordial and cooperative relations with the air branches of the Army and of the Navy. The many problems which arise in the course of the development of a new design were freely submitted to the Committee for study and advice, and the best efforts of the Committee were, through Lewis and his laboratories, brought to bear on these problems. In return, the services furnished to the Committee aircraft and engines for fundamental research, or other materials or equipment to augment their available research facilities. Among such items, the largest was an allotment of $4,500,000 for the construction of the 6 foot by 6 foot supersonic wind tunnel at the Ames Laboratory.

It may be here noted that the Executive Orders of President Roosevelt establishing the great war research organizations, O.S.R.D. and N.D.R.C., excepted aeronautic research from the fields to be covered by them, thus leaving the N.A.C.A. un-

affected and free to pursue its way without outside interference or direction. This is a striking indication of the confidence in high places enjoyed by the N.A.C.A. and its Director of Research, George W. Lewis.

Two or three illustrative cases may be cited of important results developed by the N.A.C.A. laboratories—for the first, the Low Drag Wing.

It had long been known that the boundary layer of air flowing adjacent to the wing was, during the early part of its flow, laminar in character, becoming more and more turbulent and confused toward the latter part of its contact with the wing. It was also known that the frictional drag due to the turbulent flow was much greater than that due to the laminar flow. It appeared then, as an obvious objective, to extend the domain of the laminar flow and restrict that of the turbulent flow.

Various attempts had been made to realize these ends by varying the shape of the cross section of the wing, but with no satisfying results. Theoretical studies had shown that turbulence in the air first meeting the wing would promptly induce turbulence in the boundary layer. Existing wind tunnels all had turbulence in the air stream, but in free air, in actual flight, the turbulence in the air meeting the wing is usually negligibly small.

The first step in the study was therefore to design and build a wind tunnel in which the air stream should be in a condition, as regards turbulence, as nearly as possible similar to that in free air. This was realized at Langley Field.

Tests of airfoils in this tunnel showed that the extent to which laminar flow could be carried beyond the leading edge was extremely sensitive to the form of the cross-section of the foil. In the meantime, theoretical studies had shown that the change in normal pressure along a wing surface was dependent on the curvature of the surface and this suggested the control of the pressure by the curvature. A wing section was then designed to have a pressure distribution suited to the promotion of laminar flow over the surface. This section was tested in the turbulence free tunnel with most promising results. The drag of the wing was reduced to about one-half its value with turbulent flow.

Before passing on these astonishing results, in view of the unknown effect of the peculiarities of shape in this wing section on stability, control, take-off and landing, Lewis directed the Flight Test group to take over the problem of giving this form of wing section a practical test in flight. This was successfully carried out and showed the new form to be adapted to the conditions of full-scale flight. A wing of this form was given to the Mustang Army Fighter and enabled that plane to attain a speed well beyond anything heretofore considered possible for a plane of that power.

Without detailed description, note may further be made of engine cowling, flapped wings and tricycle landing gear, all of which are now in general use, as further instances of devices and forms of construction tracing directly to the N.A.C.A. and developed under the direction of Dr. Lewis.

Reference has already been made to the cordial and cooperative relations established by Lewis with the armed services. They knew him to be wholly dependable regarding confidential matters, and they had implicit confidence in his caution and good judgment as well as in his untiring zeal in attacking their problems and coming back promptly with effective solutions.

An instance may be given of a specially confidential problem given to Lewis by the Army Air Service during a critical period in the late World War.

Strategic bombing, deep in enemy territory depends on fighter escort for the bombers. The new type of fighter plane counted on for this service seemed on the verge of failure to meet requirements. The situation was put up to Lewis by Gen. H. H. Arnold, Head of the Army Air Service, as one of the highest importance. Lewis immediately organized an all-out plan of campaign to discover the cause or causes of the trouble and find a remedy. This involved aerodynamic research at the Langley and Ames Laboratories with engine investigation at Cleveland and with flight research at all three laboratories, to test out in flight proposed remedial measures. These efforts met with success and the bombing program was laid on as planned.

These are only a few instances of many results of world-wide import, developed by research teams under the inspiration

and guidance with which Lewis effectively directed the members of his organizations.

A special feature of reports on such work, and of all reports on investigation and research, was the modesty and self effacement shown by Dr. Lewis. Literally, hundreds of specific problems relating to military and naval aircraft were attacked and solved by research teams working under his direction. But of all the reports on such work there is no signature by him. The reports themselves were signed by the man actually in immediate charge of the work and were transmitted by Lewis as coming from the Committee. At this point it may be of interest to note that the total number of documents published by the N. A. C. A. during the period covered by the services of Dr. Lewis amounts to 7,826, covering the entire domain of aeronautic science, and constituting what is believed to be an output of material relating to aeronautics unparalleled for any like period elsewhere in the world.

George Lewis was keenly sensitive to the significance and bearing on his own work of closely related sciences. For example, meteorology and aeronautics are both concerned with the atmosphere and its phenomena, and from the start of his work in charge of N. A. C. A. research, he cultivated the most cordial and friendly relations with the Weather Bureau. He was keenly alive to the utilization of meteorological knowledge and the services of the Weather Bureau in the advancement of aeronautic science.

The Chief of the Weather Bureau closes a warm appreciation of Dr. Lewis with the words:

"Dr. Lewis personally was always considerate, stimulating and helpful in his relationships with the Weather Bureau."

During the 28 years of his supervision of the research work of the N. A. C. A., Lewis saw the birth of aeronautic research and played a major role in its growth to its present commanding importance, as evidenced by the role of aircraft in World War II, and the post-war developments in military and naval aircraft and in civil air transport. Under his direction it may be fairly claimed that the N. A. C. A. has made scientific and tech-

nical contributions of incalculable value to the United States and to the world at large.

As an indication of the magnitude of its growth from its start in 1915 to the present time, note may be made that its first annual appropriation was for $5,000 while for 1945 it amounted to some $45,000,000 and for 1949 the budget stands at $48,000,000, while the number of employees reached a maximum in August of 1945 of 6,829.

The task of carrying on this great work to still higher levels must now be assumed by others, but Lewis will always be remembered as the great pioneer who blazed the way during his direction of the work of the National Advisory Committee for Aeronautics from 1919 to 1947.

In personality, Lewis was in the highest degree friendly, modest and retiring in disposition. He made friends of those about him—of the employes of the laboratories, of his associates in administrative work, of the officers of the Army and Navy with whom he came continuously in contact, and with Members of Congress with whom he had often to deal in connection with questions of Committee policy and appropriations of money to carry out the Committee's plans.

His keen interest in the work of the laboratories led him to make frequent visits to them. On such visits it was his custom to contact the employes in small groups, discuss with them their work, help with suggestions for the meeting of difficulties and above all to commend and encourage them. Such an attitude on his part naturally made warm friends of the employes.

Shortly after his relief as Director of Research (1947) and appointment as Consultant (due to a serious break in health), his associates drew up a fine appreciation, published in the *U. S. Air Service* for September 1947, from which the following extract is made.

"During the past 28 years Dr. Lewis has recruited and trained the research staff of the N. A. C. A. from a handful of workers into the present force of 6,000 people. He planned and carried through the unique research facilities of the Langley Memorial Aeronautical Laboratory, at Langley Field, Virginia, the Ames Aeronautical Laboratory at Moffett Field, California, and the

Flight Propulsion Research Laboratory at Cleveland, Ohio. He is responsible for the introduction, in aeronautic research, of variable density wind tunnels, free-flight tunnels, and high speed wind tunnels, some approaching and some exceeding the velocity of sound. He has led an outstanding technical staff from which have come a succession of advances in aeronautical science resulting in technological improvements in American aircraft of great significance to both civil and military aeronautics."

It was the same with officers of the Army and Navy with whom he was in continual contact in connection with problems on which they wished the help of the Committee. His genial helpful attitude toward them and toward their problems made for warm friendship not only with him, but helped greatly to build up and maintain a friendly entente between the armed services and the Committee as such.

The U. S. Air Service itself closed a most warmly appreciative tribute to Dr. Lewis with the words:

"We know of no man who can exactly fill the place in the American aeronautic picture which Dr. Lewis occupied so capably and entertainingly for three decades."

The professional make-up of Lewis was such that he would not permit himself to make an important decision about personnel or about a problem without exhausting the sources of information regarding the subject matter in question. With laboratories spread from Virginia to California, this resulted in a very great amount of travel and innumerable conferences with leaders and key men, with research teams and with workers individually. He also felt it incumbent on him to keep in intimate touch with the newest trends in the art through visits to military and naval flying centers, to aircraft and engine factories, and, as noted elsewhere, to Europe.

This professional strategy was of definite value to the Committee and to its laboratory as long as the research staff was small or moderate in size. Before Pearl Harbor there were some 600 people at the Langley Laboratory. By V-J Day there were upwards of 6000 in three great laboratories stretching across the country. This insistence on knowing things for himself made it difficult for him to delegate authority

to others, and in the end the burden became too great for human endurance. Without a vacation during the five years of World War II, he drove himself beyond the point of human endurance and in 1945 his heart gave warning of serious impairment. The condition grew no better and in 1947 he was relieved of his task as Director of Research to become a Consultant to the Committee. As such he continued to come to his office regularly for a half day, taking the afternoon for rest, often with a brief-case well stocked with reports for examination or problems for study.

The impairment to his physical stamina was, however, beyond repair and on July 12, 1948 he died at his summer home near Scranton, Pa.

George W. Lewis was a natural leader of men. He was endowed with the qualities of mind and character which admirably fitted him to lead American aeronautical research over the critical period of the years 1919-1947, during which aircraft came into their own as agencies of war, both military and naval, as well as for civil air transport.

As Director of Research for the N. A. C. A. he led an organization which grew from small beginnings to over six thousand people. This great aggregate of research teams in widely diversified fields of applied science and engineering, he largely recruited and trained during the 28 years of his service as director of their work. Likewise the research equipment of the three great laboratories was planned chiefly by him and his plans were presented so persuasively and effectively to the Congress that the money needed for their construction and equipment was promptly appropriated to the aggregate sum of about one hundred million dollars.

On matters of broad policy, Lewis was, of course, under the direction of the Committee. The members of the Committee, non-salaried and giving, for the most part, only a few hours at intervals to the work of the Committee, needed a man of the type of Lewis as their chief executive. He was ready to act in all matters in accordance with the decisions of the Committee, and was tireless and zealous in carrying out the broad programs of action as laid down for him.

He became well adjusted to governmental procedure, budget control, Congressional hearings, etc., and in such hearings and conferences carried himself so well and spoke so clearly and convincingly regarding the significance and importance of the proposals in question that he may properly be credited with a strong influence in determining the generous supply of funds for the needs of the Committee's plans and objectives.

George W. Lewis made for himself a unique place in the development of aeronautic science and engineering in the United States, and more widely in the world at large. His opportunities were great and he measured up to them. His name will live on always as the great leader who organized and directed the operations of the three great laboratories, the work of which has played so important a part in bringing aeronautic science and engineering from its feeble condition at the close of World War I to its commanding status at the close of World War II.

It is a proud record and one in which his family and friends may take just pride.

Dr. Lewis married Myrtle Harvey on September 9, 1908. He is survived by his wife, five sons and a daughter—Arman Kessler, Alfred William, George William, Jr., Harvey Sweetland, Leigh Kneeland, and Mrs. Myrtle Norlaine Senasack.

During the course of his life he was the recipient of many honors and of recognition in high places.

In 1934 he was appointed by Secretary Newton D. Baker to the Special Committee on the Army Air Corps (Baker Board). In 1937 he was appointed by the President a member of the Inter-American Aviation Conference in Peru, and in 1941 to the U. S. National Committee to deal with Inter-American aviation matters. During World War II he served by Presidential appointment on the National Inventors Council. In 1948 he received the Presidential Medal for Merit and O. B. E. (Hon.) from Great Britain.

In 1936 he was awarded the Daniel Guggenheim Medal "for outstanding success in the direction of aeronautical research," and in 1944 the Spirit of St. Louis Medal of the American Society of Mechanical Engineers. In 1939 he was chosen by

the Royal Aeronautic Society of Great Britain to deliver the Wilbur Wright lecture in London.

In 1934 Norwich University conferred on him the degree of Sc. D. (Hon.), and in 1944 the Illinois Institute of Technology, the degree of Eng. D. (Hon.).

He was a life member of the National Aeronautic Association and for many years served on its board to homologate aviation records. He also served on boards or committees to make aeronautical awards; notably for the Brewer Trophy, Collier Trophy, Guggenheim Safe Aircraft Competition, Guggenheim Medal, Wright Medal, Manley Memorial Medal.

He was Past President and Honorary Fellow of the Institute of Aeronautical Sciences, Member of the Society of Automotive Engineers, American Society of Mechanical Engineers and the American Philosophical Society. He was elected to the National Academy of Sciences in 1945. He was a member of the college fraternities Sigma Tau and Sigma Xi.

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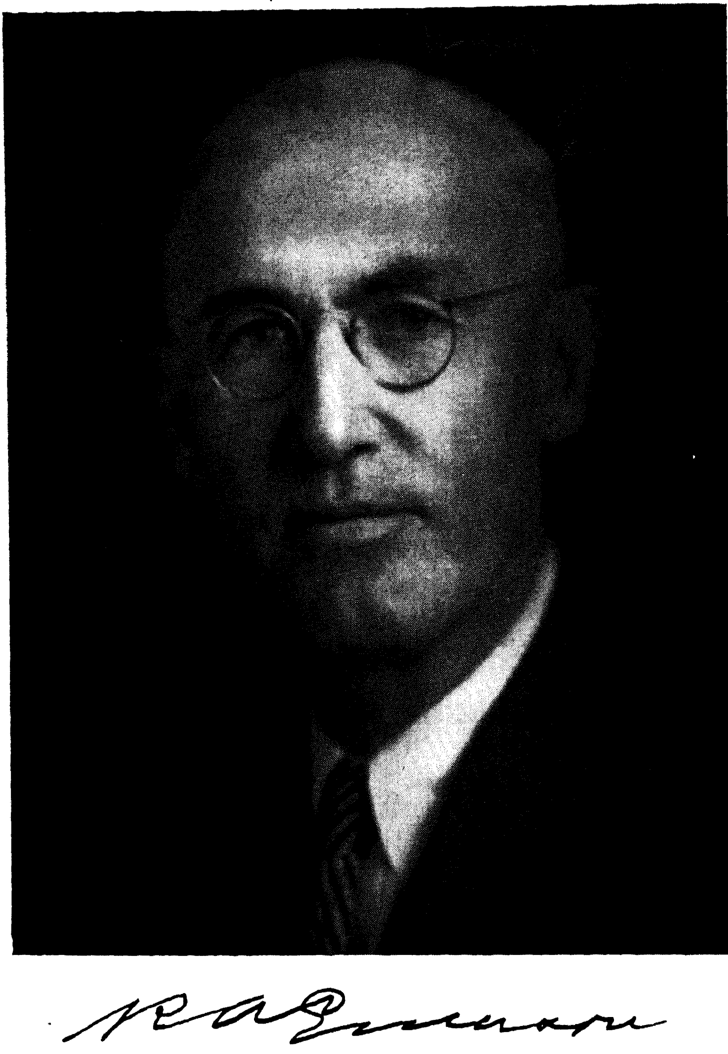

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ROLLINS ADAMS EMERSON

1873–1947

BY

MARCUS M. RHOADES

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ROLLINS ADAMS EMERSON

1873–1947

BY MARCUS M. RHOADES

Rollins Adams Emerson, born at Pillar Point, Jefferson County, New York, on May 5, 1873, was the son of Charles D. and Mary Adams Emerson. The first years of his life were spent on his father's farm in Jefferson County. However, when he was seven years of age his parents decided to leave their relatively poor upstate New York farm for the virgin prairie soil of Kearney County, Nebraska, and it was in this midwestern environment that he completed his primary and secondary education. He then enrolled in the College of Agriculture of the University of Nebraska and received the degree of Bachelor of Science from this institution in 1897. Following his graduation he accepted a position as Assistant Editor in Horticulture with the Office of Experiment Stations of the United States Department of Agriculture in Washington, D. C., where his duties were largely concerned with the abstracting of scientific papers. This sedentary occupation was not to his liking so in 1899 he returned to his Alma Mater as Horticulturist in the Nebraska Agriculture Experiment Station and as Assistant Professor of Horticulture on the college faculty. He remained on the Nebraska faculty until 1914, when he left for Cornell. In 1910-11 he took a year's leave of absence which he spent doing graduate work at Harvard University. He was awarded the Sc.D. degree from Harvard in 1913.

During his boyhood in Nebraska, Emerson's interest in natural phenomena was stimulated and fostered by his friendship with a local physician, himself a naturalist, whose friendly counsel encouraged the youthful Emerson to collect and identify the local flora. It is difficult to say how important a rôle this physician played in directing the boy's energy along scientific lines, but it was not an insignificant one. Years after he left Nebraska, Emerson often spoke with affection of this local doctor. As an undergraduate at the University of Nebraska, Emerson was a student of Charles A. Bessey, and this great teacher also exerted a profound influence on the youthful and eager student. Most important of all, at least in his formative years, was the wholesome atmosphere of the parental home. His father and mother were people of unusual ability and character who had a strong sense of civic responsibility and played a leading rôle in community affairs (his mother was

a direct descendant of Henry Adams from whom sprang the illustrious Adams family). Better schooled than their neighbors, although his father was largely self-educated, they would on occasion teach school when teachers were unavailable; both were determined that their children should have the best possible education. His parents were strict Methodists and young Emerson was reared in a well-ordered home where a high value was placed on ethical standards.

Emerson was one of the insatiably curious who are forever seeking a greater understanding of natural laws; he was a born investigator and experimenter. Even as a youth he designed and conducted experiments. His strong predilection for scientific inquiry is evident from the fact that he published a scientific paper in 1897, the same year he received his B.S. degree from Nebraska, on the internal temperature of tree trunks. These experiments, the results of which were read in 1896 before a meeting of the Nebraska Academy of Science, were begun in the summer of 1894 and continued during the vacation periods of 1895 and 1896. This first publication was a simple and unpretentious bit of experimenting yet it shows clearly the qualities of lucidness and objectivity which characterize all of his published works. A reader of Emerson's papers never is left in doubt as to the purpose of the experiment, the experimental attack on the problem, the data obtained, and the conclusions drawn. He rigorously tested every hypothesis in an admirably dispassionate way. He was not given to speculations which could not be subjected to experimental test.

Emerson's position at Nebraska, first as Assistant Professor and later as Professor of Horticulture, required him to spend considerable time on matters of practical importance to the agricultural interests of the state. During his tenure at Lincoln, a number of papers and bulletins on subjects such as mulching of garden vegetables, handling of fruit trees, etc., appeared; but he managed to find time to do a great deal of experimental work of a more fundamental nature. In a sense Emerson was fortunate in the time of his arrival in the scientific arena. The turn of the century saw the great impetus to experimental biology given by the rediscovery of Mendel's laws; physical and chemical techniques were becoming useful tools in the biologists' hands, and a new and exciting era lay ahead. Emerson began breeding work with beans, prior to the rediscovery of

Mendel's laws in 1900, with the expressed intent of learning more concerning the underlying principles of plant breeding. His hybridization of beans began in 1898 while with the United States Department of Agriculture, and in 1902 he published a "Preliminary account of variation in bean hybrids." In this paper he showed that he was conversant with Mendel's work and that he proposed to ascertain the validity of Mendel's laws in his Phaseolus material.

Emerson realized the tremendous implications of Mendelian inheritance and early embarked on a career of genetical research which was not to end until his death. He first concerned himself with the heredity of the bean plant. In 1904 appeared a second publication on bean hybrids and a number of papers dealing with inheritance of seed coat colors and other characters appeared while he was at Nebraska. His first paper on maize genetics appeared in 1910, but he used maize as breeding material in 1899 when he conducted a cooperative experiment with H. J. Webber on the hybridization of ordinary field corn, sweet corn, and Peruvian corn. His 1910 publication deals with a latent factor for aleurone color (actually the recessive gene pr for red aleurone color present in a strain with colorless aleurone) but he stated in the introduction that he had been studying the heredity of the corn plant for several years and four problems were under investigation. One of these concerned modifying factors affecting intensity of aleurone color; a second, the appearance of mottled aleurone from the cross of purple by nonpurple; a third, the nature of the red-white coloration of "calico" corn; and the fourth, on latent color factors. The year in which he forsook beans for maize as his experimental plant is not certain, but the circumstances which led to his doing so are of interest. Desirous of obtaining laboratory material illustrating 3:1 Mendelian ratios for a course he taught, he made a cross of Rice popcorn with a sugary strain and self-pollinated the F_1 plants which were expected to segregate starchy and sugary seeds in a ratio of 3:1. These selfed ears were distributed to the class with the request that the data be reported to him. To his surprise and chagrin, the students' counts showed a marked deficiency of sugary seeds. Puzzled by this deviation, he felt that he could not set aside this material until he had determined the cause of the aberrant ratio. Thus began his maize studies in genetics which were ultimately to lead to

his founding one of the most active and productive schools of genetics in the world. Many years later, in 1934, a well-documented paper appeared in GENETICS in which he showed the aberrant sugary ratios were due to linked gametophyte gene effecting differential fertilization.

In addition to the four problems mentioned in his 1910 paper, Emerson, in 1908, began experiments on the inheritance of quantitative characters in maize which were designed to test whether or not these differences were due to numerous factors inherited in a strictly Mendelian manner. His results, together with similar data obtained by E. M. East, were published jointly in 1913 in what constitutes, even today, one of the best papers on the inheritance of quantitative characters. Emerson's interest in this problem never lagged and for many years, up to his death, he studied the inheritance of ear row number. Unfortunately his vast assemblage of data on row number was never published. It is to be hoped that some method will be found whereby his extensive data can be made available.

Emerson was called to Cornell in 1914 as head of the Department of Plant Breeding, and it was there that his most important work was accomplished. Two of his students, E. G. Anderson and E. W. Lindstrom, accompanied Emerson to Ithaca and they were soon joined by others. Many prominent geneticists studied with Emerson at Cornell and his laboratory became known throughout the world, attracting many foreign students. Except for his first years at Cornell, he never engaged in formal teaching. His time was divided between administrative duties as Head of the Department of Plant Breeding and research in maize genetics. His method of handling graduate students was as effective as it was unique. He looked upon graduate students as mature individuals who should not be led by the hand but who should be given the opportunity to develop their own ideas. Above all he tried to encourage independent thinking. Although always available when students sought his help, he felt that the initiative lay with the student. When a new student appeared, he would usually assign him some routine problem. He often remarked that he found this a very satisfactory method, because if the student were good, he would soon find a more interesting and exciting problem for his doctoral dissertation, while if the student were mediocre, it didn't matter what kind of a problem he had.

Emerson was the spiritual father of his students and the impress of his personality was left in part upon all who studied with him. His contagious enthusiasm, his prodigious energy, his absolute integrity and objectivity were such that all who were intimately associated with him caught in some measure these attributes of the man. Close personal ties bound him to his students. He once remarked that he looked upon them as sons. It is certainly true that he took almost as much pleasure in the achievements of his former students as if they were of his own flesh and blood. Graduate students are prone to be hypercritical of their professors, seizing upon any real or fancied weakness with a zest which must be disconcerting to their elders; but Emerson's students never spoke of him save with respect and affection. Standing six feet in height, possessing a powerful physique, Emerson was a fine figure of a man. His fine personal qualities endeared him to all who knew him.

Emerson was completely absorbed in his scientific work. In July of 1947 he underwent a major surgical operation which disclosed that he was critically ill. Although 74 years of age he made a remarkable, but temporary, recovery and during the latter part of the summer and the early fall months did as much field work as the average man in good health. His condition was so weak that he would work for a short interval, then rest in his car until his flagging strength returned, whereupon he would again resume his tasks. Finally he was forced to his bed from which he never arose. But even in his last days his mind was occupied with genetical problems and he spoke of his work with his usual enthusiasm and fire. Truly his spirit was indomitable. He was a man in every sense of the word.

It might be said of Emerson as of Morgan that his greatest contribution to science lay not so much in his own research, however significant, but in the great influence he exerted as the inspiring leader of an active and productive school of genetics where young and promising students found a stimulating intellectual atmosphere. Nevertheless Emerson's researches were of the highest order. His masterful analysis of plant color inheritance, published in 1921, did more than any other single paper to establish maize genetics on a sound basis. His demonstration that pericarp variegation was due to a mutable gene was the first proof of such a genetic basis for variegation. His studies on sex expression, on quantitative inheritance, on Zea-

Euchlaena hybrids, on multiple alleles, were all milestones of progress. More than any other single investigator he was responsible for the determination of the ten linkage groups of maize.

Emerson's influence among students of maize genetics was widespread. By general consent he was the dean of maize geneticists and men such as Stadler, Jenkins, Mangelsdorf, Brink, Edgar Anderson, Hayes, and Jones, who were not his students, came to counsel with him. The high esteem in which he was held by his colleagues was such that he was able to organize the Maize Genetics Cooperation and the Maize News Letter. To this News Letter, published annually, come unpublished data, progress reports, and scientific notes and comments from many laboratories. Seed stocks are both maintained and distributed by the Cooperation. This magnificent cooperative effort, which did much to advance the progress of maize genetics, was Emerson's creation.

A remarkable incident, since it involved two men destined for leadership in genetics, occurred in the pre-Drosophila days of 1909 at a meeting of the American Breeders' Association. T. H. Morgan appeared on the program with a paper entitled "What Are 'Factors' in Mendelian Explanations?" in which he expressed his skepticism of the already then commonly accepted belief that alternative conditions (alleles) undergo segregation to form two kinds of germ cells in equal numbers. Morgan stated that "equal numbers of the alternative conditions are not always present in each individual." In the same address Morgan further expressed his position with the following criticism: "If one factor will not explain the facts, then two are invoked; if two prove insufficient, three will sometimes work out." Emerson immediately followed Morgan on the program and presented a paper on "Factors for mottling in beans" in which he showed that the genetic data could not be accounted for by a single factor pair for mottling but that two factor pairs offered a satisfactory explanation for the data! Emerson was among the first to see the great promise of this new field of biological investigation. Later, in 1910, Morgan fully accepted the Mendelian interpretation of heredity and became the leader of one of the most brilliant schools in the history of all biological science.

Although most of Emerson's work was in theoretical genetics,

he was genuinely interested in the application of genetic methods to plant breeding. In the 1920's he developed an anthracnose resistant pea bean and in his later years obtained some greatly improved strains of celery and melons. He seemed to take as much pride and pleasure in his successful breeding of celery and melons as in his genetic studies. As a matter of fact one of his most striking characteristics was the whole-hearted enthusiasm with which he threw himself into every undertaking, be it genetics, plant breeding, hunting or bowling. He felt that if anything was worth doing, it was worth doing well.

Many honors came to him. He was a member of the National Academy of Sciences, the American Philosophical Society, the American Society of Naturalists, of which he was President in 1923, and the Genetics Society of America, which he served as President in 1933. He was a charter member of the American Society of Horticultural Science and a fellow of the American Association for the Advancement of Science. He was affiliated with the American Association of University Professors and the American Genetic Association. He was a member of Phi Beta Kappa, Sigma Xi, Phi Kappa Phi and Gamma Alpha. He served as Dean of the Graduate School of Cornell University for six years (1925-31). He was faculty representative on the Board of Trustees of Cornell University from 1925-27. The University of Nebraska awarded him the LL.D. degree in 1917. Emerson, however, wore these honors lightly; he was a modest man without pretense.

He made a trip in 1923-24 with F. D. Richey to the principal maize growing countries of South America for the purpose of collecting indigenous South American varieties. In 1935 he went to Yucatan at the invitation of the Carnegie Institution to study the probable food plants grown by the ancient Mayan peoples. He was a delegate to the Seventh International Genetics Congress at Edinburgh in 1939.

He was married to Harriet Hardin, on May 23, 1898. Four children were born of this union, all of whom survive. His eldest son, Sterling Howard Emerson, is now Professor of Genetics at the California Institute of Technology. All who had the privilege of visiting the Emerson home know of the strong ties of affection which bound this family together. Mrs. Emerson preceded him in death by several years. Professor Emerson passed away on December 8, 1947.

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